



C U R R I C U L U M V I T A E

Charles E. Kellogg

Volume 2

1948 - 1958

1948. The Department officials agreed that I should accept an invitation to take part in a Commonwealth Conference on Tropical and Sub-tropical Soils being held at Rothamsted in June, with

supplementary visits at Oxford and Lyndhurst. *(Ogg had, at first, spoken of a conference on "Colonial soils".!)*

Early in the year Dr. Ignatieff and I got our committee of the ^{young} Agricultural Society together and outlined the proposed book on the efficient use of fertilizer and chose authors for the several sections. The big problem was to get our American writers to take account of soil conditions they had never seen so we developed a scheme for getting comments and ideas from a great many people in other parts of the world.

This year I started the practise of inviting one representative from among the professors of soils in each of the four land-grant regions to our national staff conference. This took some doing because some of the experiment station directors wouldn't even pay the expenses of their people and they asked the Soil Survey to do it; and the Mid West boys wanted to have three or four representatives from

this one group of states. Then too, we invited a few from other federal agencies. I was anxious to get a very broad representation of competent opinion because I wanted to get all of the vital committee reports completed and tested by the autumn of 1949. (The only way I finally got out of this haggling over expense accounts was to serve them notice about 4 or 5 years later that we simply would not pay the expenses of people not on our Soil Survey staff.)

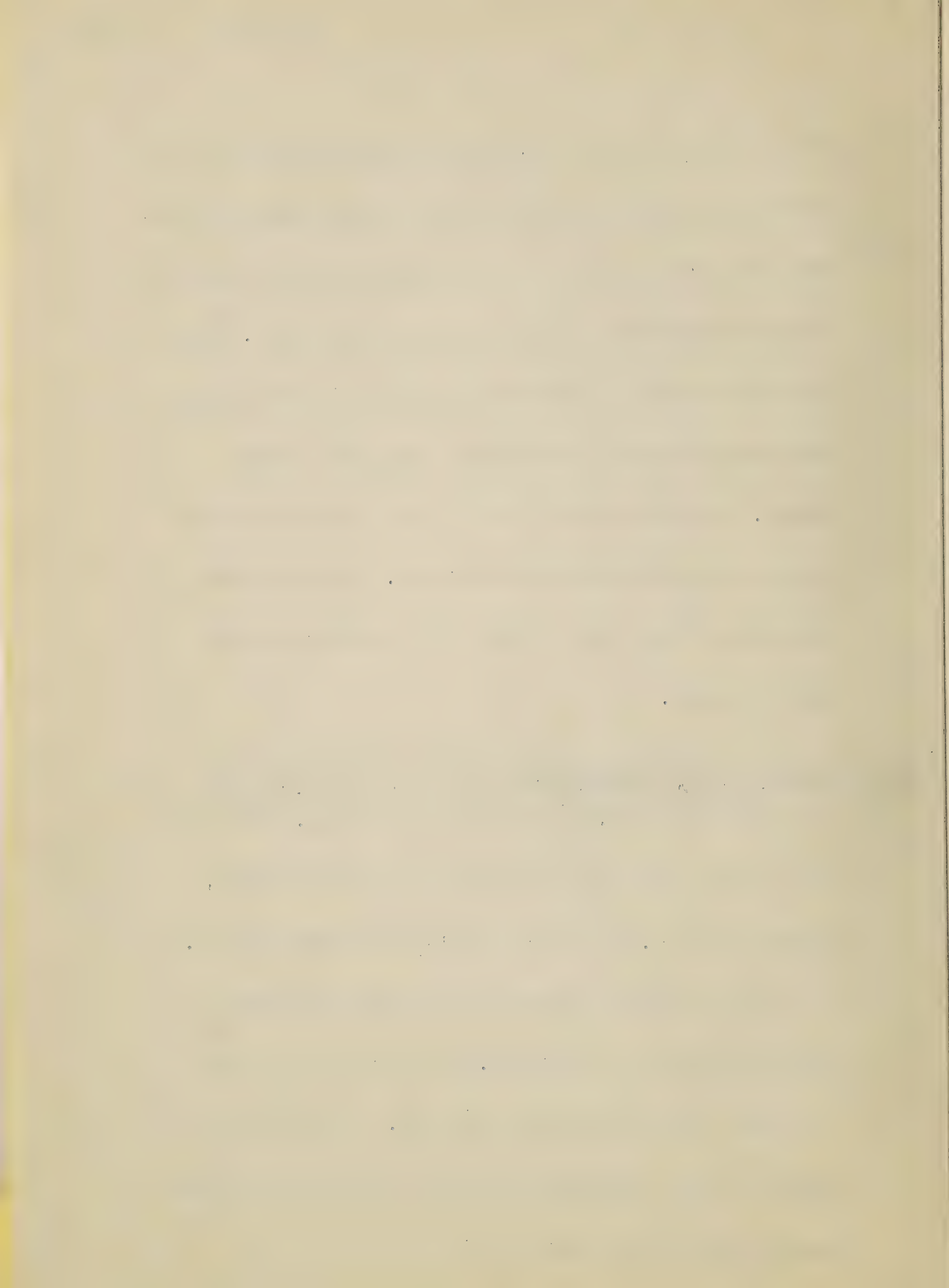
From January 11 to 24 I spent my time in Knoxville working on fertilizer policy, test-demonstration plans, and agricultural organization within the Tennessee Valley Authority. I loved McAmis but he was an extremely poor administrator and had a deep-seated prejudice against the use of nitrogen fertilizers. I did a lot of work on the nitrogen problem based on conferences with both chemists and agriculturists. McAmis had no use for the nitrogen program. He had been brought up on getting nitrogen through legumes and always spoke of ammonium nitrate as "sweet wind". But I could see that it had a great place on pastures, and especially on corn now that we had the new hybrids. Instead of having yields of 15 to 35 bushels

per acre on erosive hilly soil, corn could be grown continuously on the best soils for it with yields of well over 100 bushels per acre. Such a corn crop gives nearly complete protection against erosion and leaves great quantities of organic matter within the soil. With the price of nitrogen coming down this was bound to revolutionize agriculture and permit high production with greatly reduced erosion. Also there was the need of arranging for the fertilizer plant to have a truly national significance. Curtis was also a bit opposed to the nitrogen program but I was able to sell the Board as a whole.

SN
P. 328
WET
I also had to do a good deal of preparatory work in order *a useful paper on tropical soils* and for *M. Cline* to have a useful paper *for the conference in Britain* Marlin Cline

came down and we had a staff conference during which the term, "Latosol" was born. (I am sorry I didn't use "Laterosol instead.)

Early in February I spoke to the Association of Southern Agricultural Workers in Washington on the contribution of the Soil Survey to the agriculture of the south. This gave me a chance to show the relationship among the fertilizer work, land-use planning, and the Soil Survey.



I don't think I ever had more writing going on. Iver Nygard and I were working on the Soils of Alaska. I had begun the bulletin *my sail week in* on the Congo and had my speech to fix up for Britain as well as several other speeches.

From February 14 to 26 I spent my time in the Tennessee Valley, partly at the fertilizer plant at the Shoals and partly at Knoxville.

Finally McAmis said to me that he should leave his job. I got Mr. Bass *and* ~~on~~ the Board to accept this immediately and arrangements were made

for White to take his place and for Joe Moon to become Assistant Director. McAmis never was friendly with me after that and ~~soon~~

about 2 years later retired from TVA. I was very sorry about this but he simply could not

handle the job. He had many valuable insights but ^{*he*} also some bad

prejudices. He always talked ^{*in parables*} to his staff and ^{*to*} every one else ~~in~~

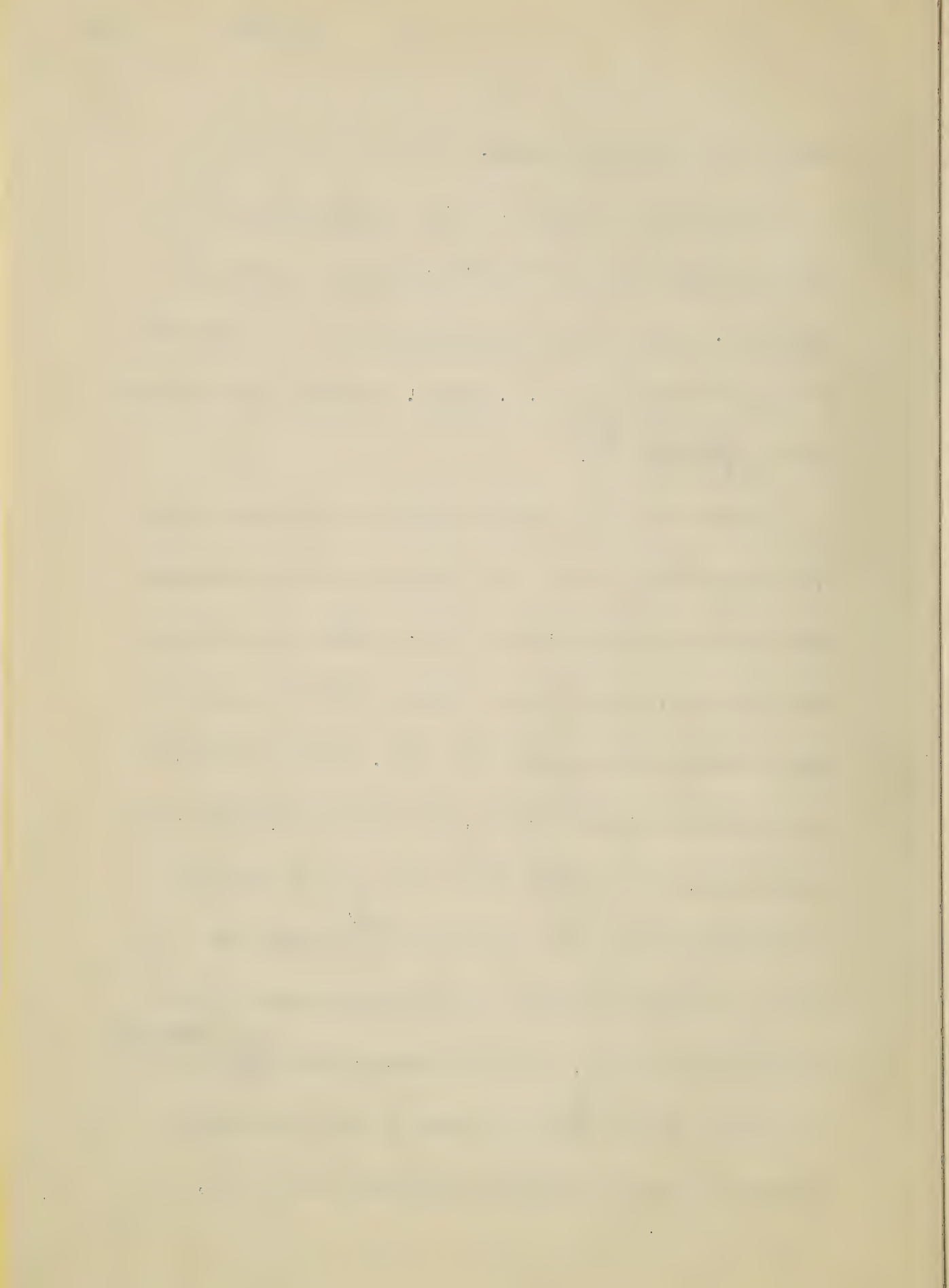
~~parables~~ so that communications were poor and uncertain. And on the nitrogen work he wasn't above fudging the data a bit.

During these two trips in January and February I did an enormous amount of work and got several other people to help on some of the public relations and economic aspects of TVA's

chemical and agricultural programs.

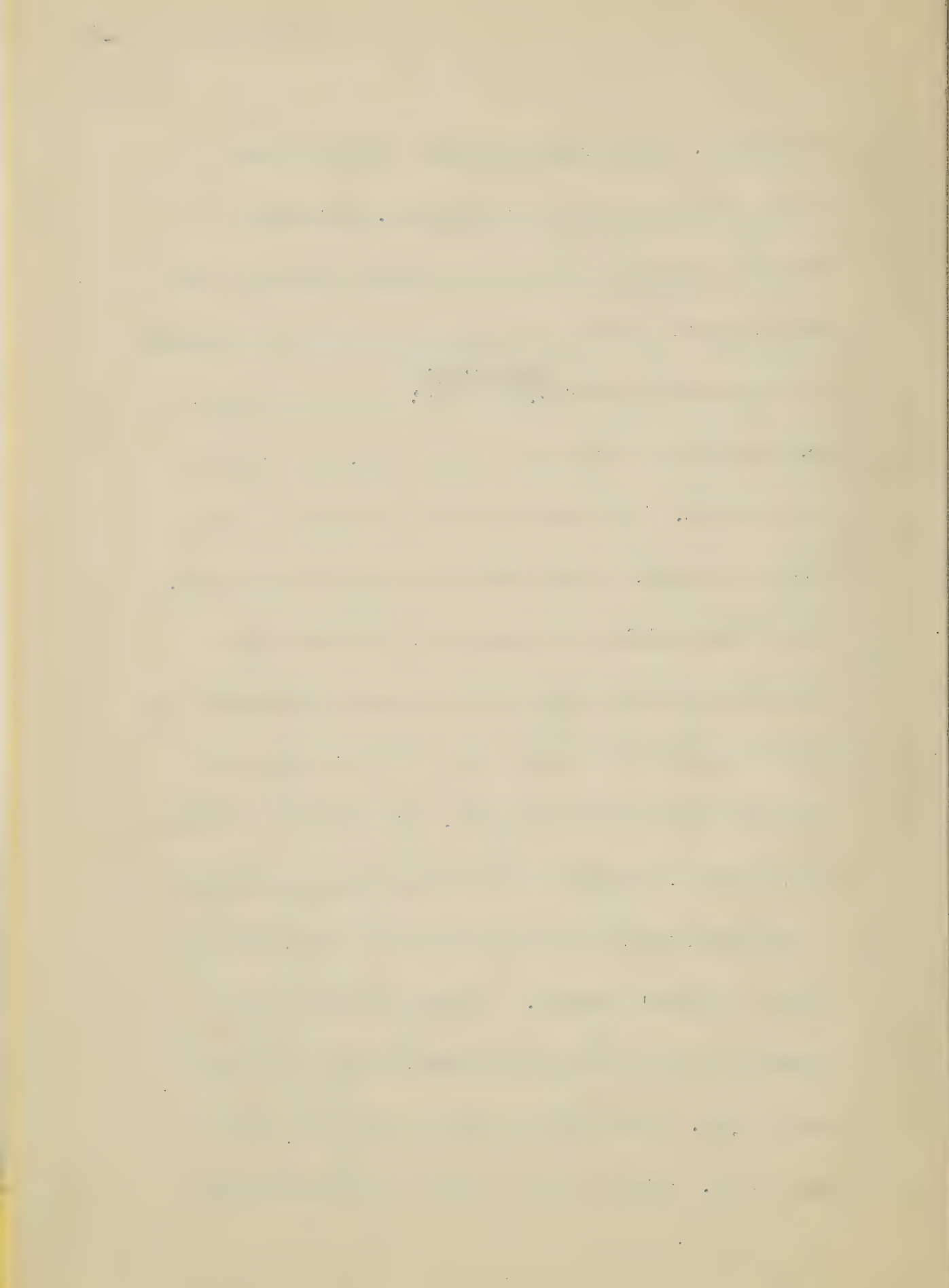
Before I went to the Valley, I asked the girls to look up the plane schedules and boat sailing^{to} for England so I could make a selection. When I returned, they simply showed me my reservations for going and coming on the S. S. America! No doubt I needed the rest and it was ~~showing~~^{to} a bit.

In February and March hearings were held on three crazy bills. H.R. 4150, H.R. 4151, and H.R. 4417. Near the end the concentration was on the so-called Hope bill that varied somewhat from the Jensen and Cooley bills. These bills were promoted by the SCS in order to make an empire for Hugh Bennett and his boys. They went up and down the land with the ridiculous slogan, "The United States needs to have a land policy. We have never had one." Actually, of course, the United States had had a firm land policy, ^{the} that productive land should be in private ownership. Of course Extension and the Farm Bureau were much opposed. One of the incidents that hurt ^{the "cause"} SCS was the fact that one of the ^{SCS} boys in Missouri lost his briefcase, which contained many detailed letters and questions to use in talks,



conversations, and conferences to support the Hope bill and discredit Extension and about everybody else. Obviously it represented a dictatorial, bureaucratic campaign concocted in the Washington office of SCS in cooperation with the National Association of Soil Conservation District ^{or actually not} Supervisors. Of course this plan for empire included taking over the Soil Survey, which was a big goal of Bennett. But he went so far that he antagonized all the land-grant colleges, the Farm Bureau and other farm organizations, and most other agencies of the Department. This whole affair created uncertainty and bitterness that was wholly unnecessary. But perhaps it was good that he did go so far since his extremism made failure of these bills inevitable. This affair is well covered by Dr. Charles M. Hardin in his book, The politics of agriculture.

Ray Heinen joined the staff of the Bureau March 8, primarily to help with Salter's speeches. It turned out that we worked together very closely for about five years. Salter would have a speech to give. Sometimes he would have a title in mind and sometimes not. Ray would come over and we would kick some ideas



around for an hour or so. Then he would go back and make a rough draft that I would mark up. Then we would have another talk. This made things a lot easier for me. Ray never wrote any speeches for me but he gave helpful criticisms on second and third drafts. We also cooperated on speeches for the Research Administrator, the Secretary, and others.

From March 14 to 20 ~~that year~~ we had our Soil Survey staff meeting, now called the work-Planning Conference of the National Cooperative Soil Survey, at the Statler Hotel in St. Louis. My Canadian friends were there and Stephens from Australia. Moon came for the last time. We started a general pattern that continued for many years. The first day we had a plenary session and then the committees met in evenings and during the days. For about one and one-half days we went ahead with the committee reports and had a final session, late Friday or Saturday morning. As the attendance grew, they became increasingly difficult to manage without waste of time.

On the side, I had been promoting a National Phosphate Committee that would not be dominated by Frank Parker. This worked out quite

well with Dr. W. H. Pierre taking the leadership. The Committee continued for some time with a final national conference and publication about 1951.

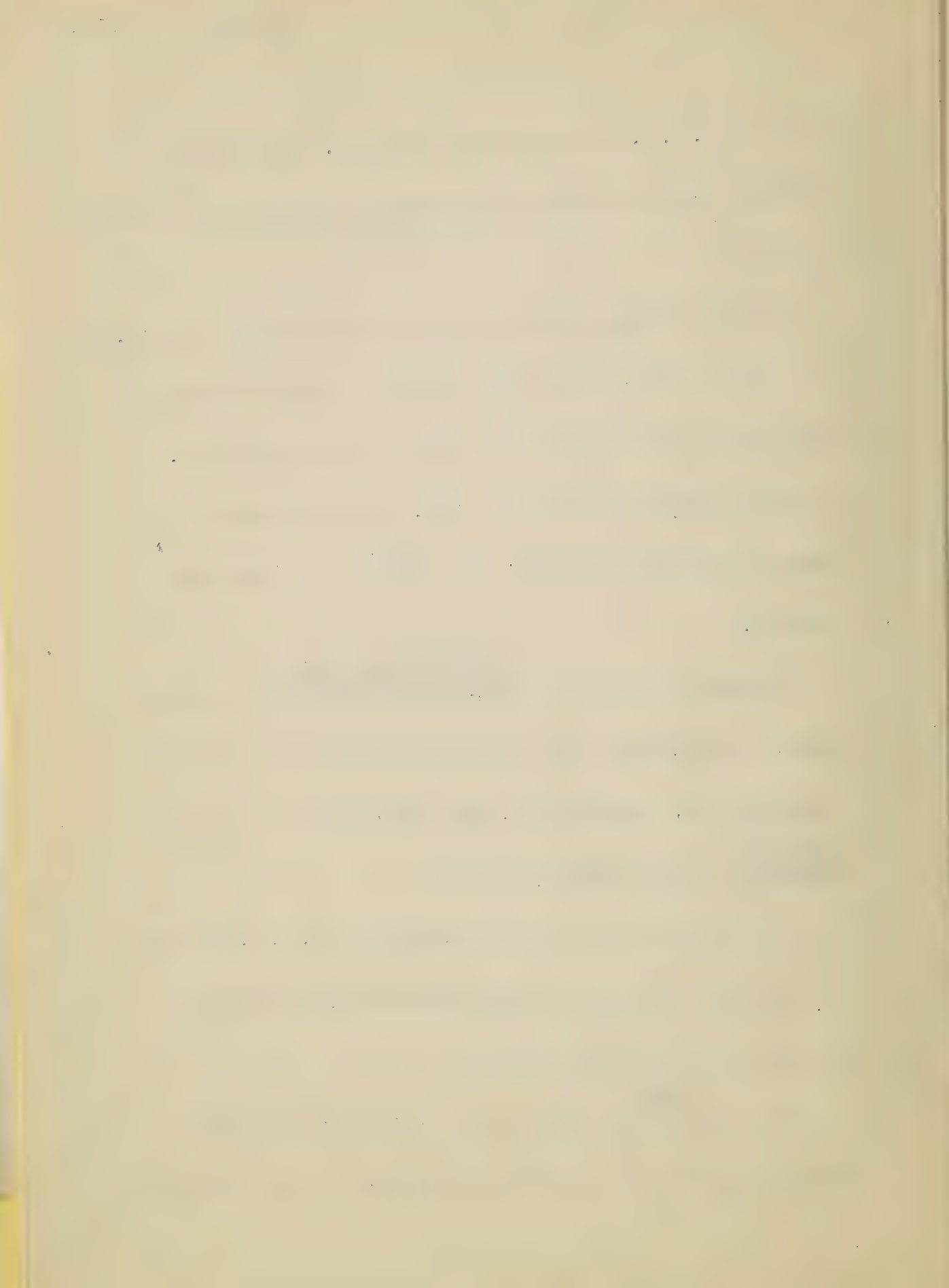
April 7 the changes were made in TVA and Joe Moon went with them.

In the middle of April, C. G. Stephens of Australia ended his field trip in Washington and spent some time with the staff here. He was very enthusiastic about his visit. I had high hopes for Stephens but in the end he didn't quite ^{make} the high grade I had expected.

Besides my own writing I got ^{last} ~~much~~ ^{manuscript} ~~to react to~~ from Jim Rorty. He was working a good deal with TVA bulletins and also on a book of his own. Then, of course, Major Ignatieff and I were pushing ^{ahead} ~~the book~~ on the FAO fertilizer bulletin.

In May my files show that I was writing to Prof. G. W. Robinson and complaining "that being both an administrator and a soil scientist is a very difficult trick."

^{Ray}
Also in May, Hockensmith of the SCS brought out a new little booklet, "Guide for soil conservation surveys", By making some use



of the committee reports of the Soil Survey staff the classes of soil characteristics were slightly ^{no}improved but the basic fallacies of complex symbols and uncontrolled legends remained, so *SCS "surveys" were* that the ~~work was~~ as bad as ever.

Near the end of May I received two or three copies of the "Scientific Monthly" containing my paper, Conflicting doctrines about soils, which was destined to have an interesting history in several languages.

I left for Europe on June 2. The trip at sea was very restful and I got some writing done. I met *Harry Varley* *seemingly* an interesting man with lots of money, ~~by the name of Harry Varley~~. He was in the advertising business in New York. He had *along his* wife, much younger than he, who took quite good care of him. He did some quite good original writing but published it privately and gave copies to his friends.

We docked at *I* ~~I got to~~ Southampton the morning of June 9 and took the train into London. I went over to the St. Pancras Station, got my ticket, and looked at the magazines. The Atlantic edition of "Time" for June 7 had a big story about my paper in "Scientific Monthly." Contrary

for the Amsterdam Congress planned for 1950. I gave an important paper here with a very long title, Preliminary suggestions for the classification and nomenclature of great soil groups in tropical and equatorial regions.

I got back from Britain about July 8. The big table back of my desk in the office was stacked high with letters about, Conflicting doctrines about soils. I had several thousand requests for reprints but had ordered only 200. The *Production and Marketing* ~~Agricultural Adjustment~~ Administration made copies for all their offices. Many others made copies or reviews and I helped with a few short versions. It was translated into several languages, *(and, in 1950,)* FAO reprinted it and circulated it widely in the world.

July 11 Lucille and I drove to Ithaca, New York for a meeting of the collaborators of the Plant, Soil, and Nutrition Laboratory and I also used the occasion to go over our cooperative work with Bradfield and others at Cornell.

Later in July Georges Aubert came. I had given Walter Lyford around \$150 to give Aubert when Walter met him in New York. The French had taken me literally and Georges had only five dollars in his pocket when he landed in New York. We had arranged a system of tours so that he was passed from one group to another and had almost no expenses for transport. I had also arranged with several universities to give him honoraria for lectures, although some of these he had to get to by train after the beginning of the school year. ^{Later} ~~in~~ ^{Fort} Collins ~~later~~ I gave him some more money and he continued his travels by car to the southwest and then through the south to Washington.

In early August we had a vicious attack on one of our best soil scientists by an SCS boy whom I suppose was looking for favors from Bennett. This fellow was called Claude Fly and was the state soil scientist for Kansas. We had worked out a three-way cooperation between the Soil Survey, the SCS, and the state experiment station. Arvad Cline was the party chief. Things were going fairly well and then all of a sudden Fly went to Prof. Throckmorton and told him that

History

the soil maps were inaccurate and unusable in farm planning, and that the Soil Survey had not met its commitments. Happily, Arvad had anticipated something like this from Fly and had kept accurate records of each man's work with dates. Throckmorton became ^{upset} ~~very~~ angry at this story and came with Fly to visit the county. Arvad had complete records, which showed that only the Soil Survey had met its commitments. All of the SCS men had been trainees and had been moved out as soon as he had trained them. The local SCS farm planner said the maps were fine for farm planning. Then Arvad said, "Now let's go out and look at the boundaries." By this time Fly had left and Throckmorton couldn't find anything wrong with the boundaries. People had been suspicious of ^{Fly} ~~him~~ before, but ^{Carter had warned me about him} ~~no~~ one trusted him after that. ^{but} Arvad got a raise immediately.

Also in August I was asked to review one of the worst books dealing with soils and soil conservation that was ever published - The road to survival by William Vogt. This had to be the end of a cycle, beginning with the bulletin, Soil erosion the national menace, by Bennett and Chapline, supplemented by increasingly extreme statements

by both Bennett and Lowdermilk. Russell Lord carried the falsifications

a little further and in better English. Stuart Chase had another

bad one, ^{and} ~~that got quite a lot of publicity.~~ There were several more,

but ^{By} then there was so much money in this kind of scare stuff, and

so few people knew anything about it, that folks in Europe wanted to

get in on it. The book by Jacks and White, called Rape of the earth

in England and Vanishing lands in the United States, was ^{the money maker} next in

line. I had reviewed this earlier. After Jacks' book came one a

little worse in excellent French by Harroy, called Afrique: Terre

qui meurt, published in 1944. I saw Harroy in the Belgian Congo

in 1947 and asked him why he wrote such a book. I said, "You know

that there is mighty little significant erosion in the Congo." He

replied, "Yes, but you have to exaggerate to get ^{the attention of} government administrators,"
to act."

Now The road to survival had gone even beyond the one by Harroy.

In reading the book I found only one true statement: Somewhere ^{in the book} he

writes that the Soviet Union had a continental climate. But everything

else that I knew anything about, within the United States or

elsewhere, was wrong.

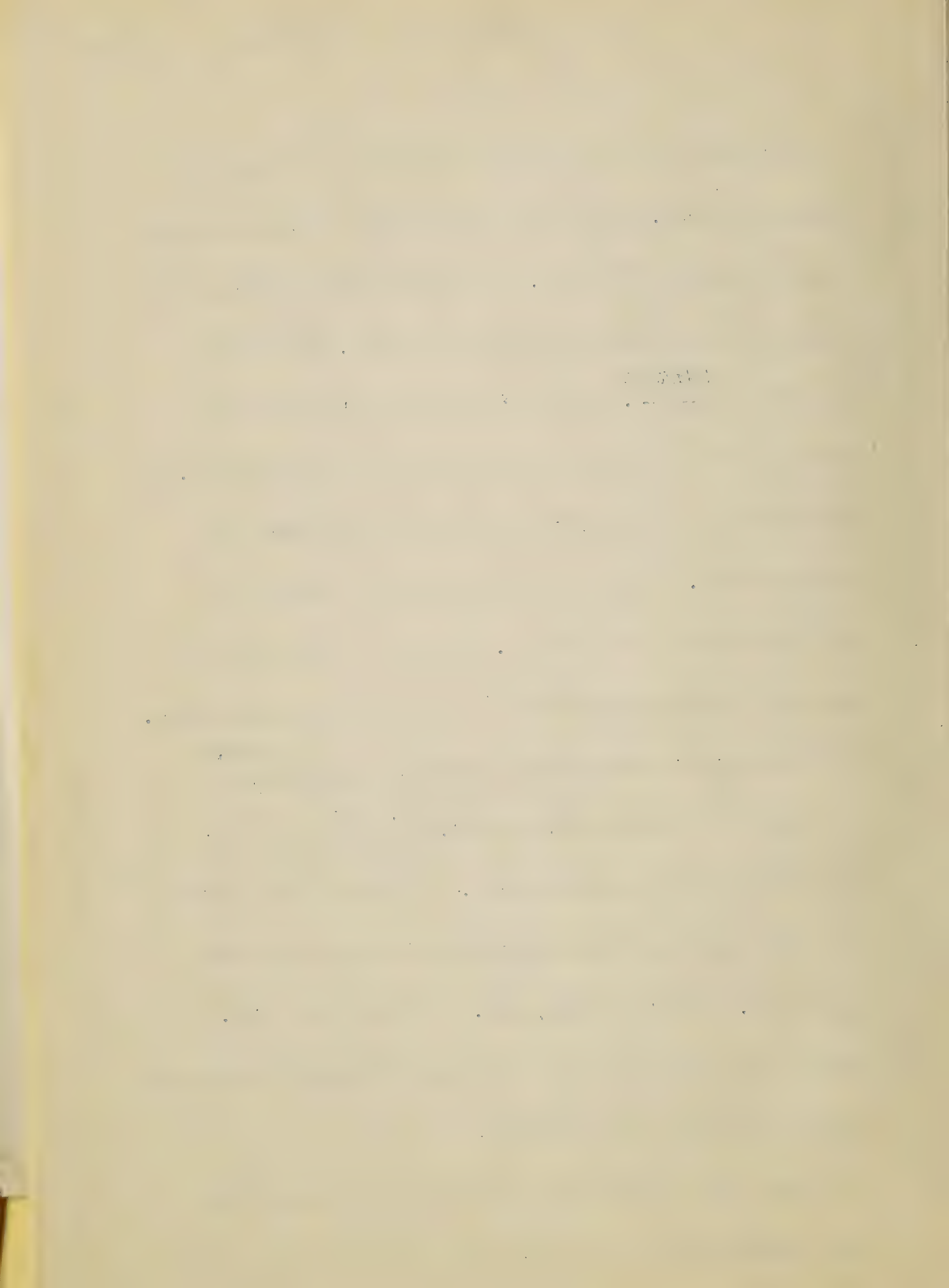
August

I refused to review this book on the basis that it would just help sell the book. People would want to see it in order to find out what got under my skin so much. Then I got an order from Secretary Brannon to review it for his personal information. This irritated me a bit ~~but I did so~~ *urated it*. The boys in the Secretary's office tried their best to get me to let it be published but I would not agree. I found out later that they had copies made and circulated them under the table. I think one probably got back to Vogt but he never said anything to me about it. Much later I found that a copy with no name on it was posted for reading in the Harvard library.

Aug
This review is worth quoting as follows: (Insert 345a)

Fort
August 21 I left for meetings in ~~Fort~~ Collins with a stopover in Chicago to talk with Charles Hardin. The campus at Fort Collins was a poor place for the meetings because of the very noisy wooden dormitories. I didn't get much sleep. Aubert had a good time. I attended many of the programs but spent much of my time in conferences with our state cooperators and others.

I went from Fort Collins to New York City where Lucille and Mary Alice had gone for a holiday. I called on Harry Varley and he



CONFIDENTIAL

Review of: Road to Survival by Vogt

Perhaps some will say that the author's intentions in this book are praise-worthy: He is trying to make the simple masses aware of soil erosion and the impending catastrophe. Although such books may make people emotionally conscious of soil, they confuse rather than aid their understanding. This is wholly a "seare" book--a trashy panic-maker with nothing constructive. It is discouraging indeed that such a book could be taken seriously by responsible people.

Here and there, one may find some facts. The book points out that one cannot carry the methods developed in temperate regions like the United States into tropical areas like Latin America. He has a few sentences suggesting the need for research; and it is true that only a little is known about tropical soils. Elsewhere, however, he says that the soils of Africa have been studied extensively. This is untrue. He emphasized the damage that has been done by American businessmen in Latin America who have followed policies inconsistent with the Good Neighbor Policy of the American Government. In the discussion of Asia he calls attention rightly to the great extremes in climate due to isolation from tempering bodies of water. In more than one place he points out the difficulties that have arisen from technological advances in medicine--in life-saving--that were not accompanied by parallel advances in agricultural and industrial efficiency. But one gathers that he suggests stopping medical work, or enforcing birth control by some unexplained means, rather than developing scientific agriculture and industry to feed ourselves.

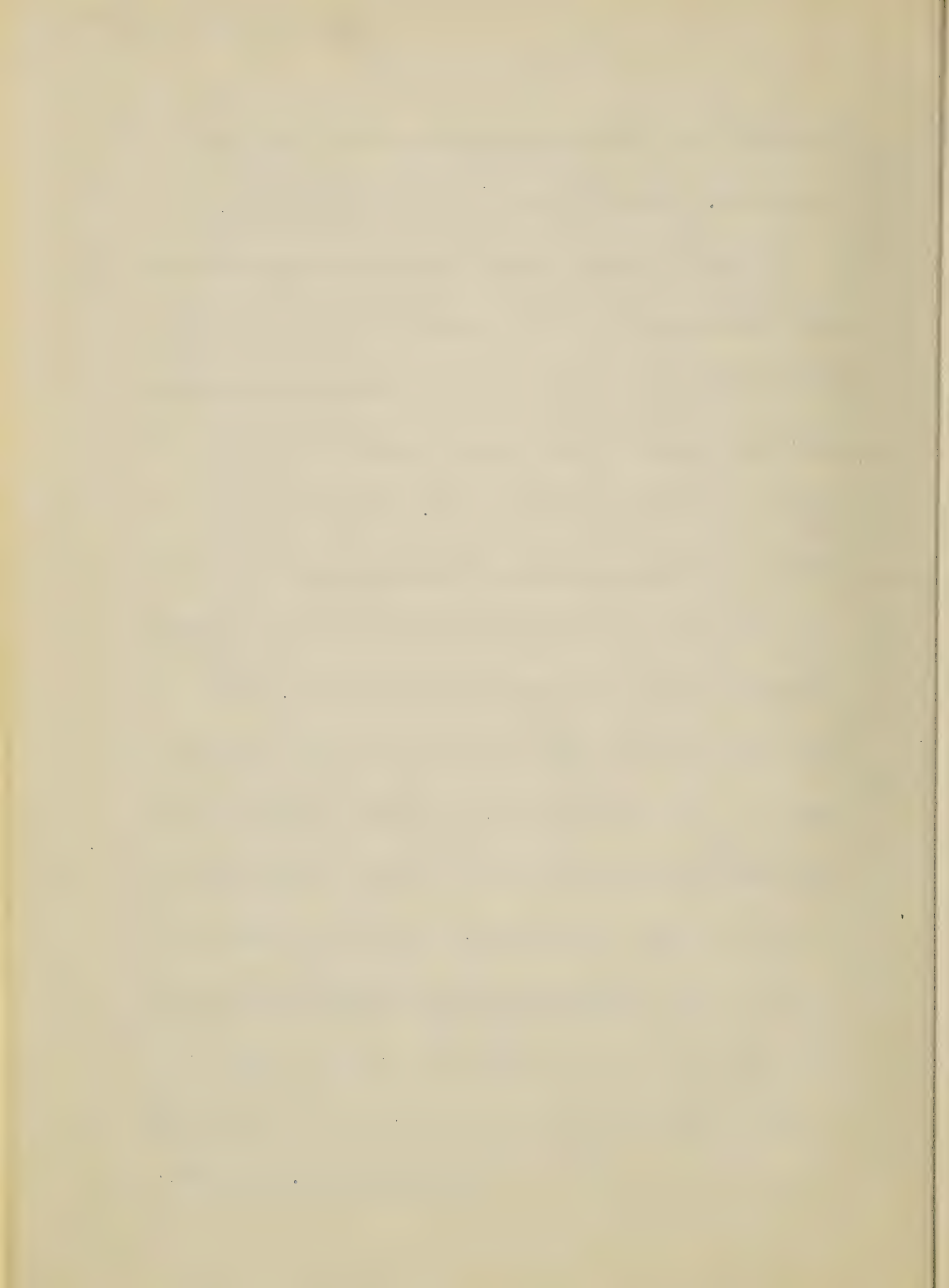
One might find a few more examples of true statements, but not easily. The book is a veritable orgy of fact, untruth, plausible statements for the wrong reasons, guessing, hate, idol worship, and sweeping false implications. With overwhelming conceit, the author alone sees clearly the great harmonies among the

Sept.

offered me a fine position and took us all to a very fancy place for luncheon. We got back home about the last day of August.

Jim Roe of "Successful Farming" had been after me for some time for a popular piece on the food potential of the world. In fact, he had sent me a bon voyage telegram to my boat in New York in June that ended, "And don't forget the piece you are to do for me." Somehow I got it done and sent it to him. It was printed in November as, Who says the earth can't feed her people?

September 10 I went to Schenectady, New York to give a broadcast on General Electric's Farm Forum about food potentials. While waiting for my train to Chicago I worked up an outline for the UN speech and mailed it to the office. I met several others in Chicago going ~~along with me~~ ^{also} to the meeting of the American Farm Economic Association at Green Lake, Wisconsin. I had a good reception for my paper on Food production potentials and problems. This turned out to be quite a good meeting but obviously a lot had to be done to integrate production research and economics. I remember getting very irritated at the figures from a California farm study. The speaker



said that only one to three percent of the farms had more than 1,000 acres of cropland. He intended this to be misleading instead of indicating the percentage of cropland in farms of over 1,000 acres.

~~They had~~ *A* ~~very bad~~ section on the economics of soil conservation ~~was bad~~
except for one paper by Heady. People were still trying to make

some kind of economic sense out of contrasting "conservation practices" with "production practices." This fallacy still lingered for a few more years.

I got back home about September 17 and I got to work again on the bulletin for UNESCO, Food, soil, and people and my paper for the UN meeting in New York.

The last day of September I started out on another long trek doing what writing I could on the train. I stopped at the University of Chicago for conferences with a group of economists and then went on by train to Manhattan, *Harvard* I had conferences there with Harold *Meyer* about the cooperative work.

October 3 I drove in a government car south into the Flint Hills and then we worked our way up to Lincoln, Nebraska, where I had

conferences with our people and with those at the university.

Fortunately for the Department of Agriculture, Lambert had left to become Dean of Agriculture at Nebraska. He wanted a first-class soil scientist but didn't want to pay over seven or eight thousand dollars. As a result he never did get one! October 5 I took some time out to dictate a statement for Dr. Black to use in one of his many reports for the Planning Association. I also had some very interesting talks about root-soil studies with Prof. Weaver. He was the great expert in this field and was affectionately known as "The goundmole of the Great Plains."

From Lincoln I went to Boston and spoke at the Littauer School at Harvard. Dr. Black never liked people to know the subject of the seminar in advance. This was hard on the speaker but I supported the scheme because no ghost writer ever penetrated his seminars.

I left Harvard in the afternoon for Durham, New Hampshire. I visited many of the staff at the University and made a speech on Soil and food for the world as part of the program for inaugurating ^a the new president.

Shortly after I returned from New Hampshire Aubert came to stay with us, although he was in and out to make his university lectures.

One day a man came into my office from "Time", who wanted to do another story on the food situation. I gave him a lot of material, including, I think, my confidential review of Vort's book, and maps. But I told him, "If you bring my name in any way into that article I shall simply raise hell." "But" he said, "I need a name". I said, "That's easy, go talk with Dr. Salter and ask him for ^{his} a picture and you'll have a name." All my friends knew where the material came for the story Eat hearty in "Time" for November 8, 1948. But nobody could prove anything.

Near the end of October I made a visit to the United States Salinity Laboratory at Riverside, California and took part in the collaborators' meeting. This gave us a chance to talk over the necessary cooperation between the Laboratory and the Soil Survey. To change the pace from salty soils, I gave an evening slide lecture on the Congo.

~~After three days at home I went to the Tennessee Ag~~

After three days at home I went to Memphis and gave a lecture to the Tennessee Agricultural Extension Workers.

Another screwball book had come out earlier on soil conservation called, The plundered planet by Fairfield Osborn. One night at the Cosmos Club one of the fellows who "worked" with him asked me if I had read it. I told him I hadn't. So a day or two later Osborn called ~~me~~ from New York and asked ~~me~~ if I would read it and tell him what I thought of it if he sent me a copy. I agreed. The book wasn't any good, in fact it was extremely misleading, so I sent him a ~~three-~~ or four-page letter documenting this ^{opinion} ~~opion~~. After that he left me alone.

Dr. C. P. Barnes left us for a position in the Research Administrator's office in November. It was very difficult to find a good replacement until I finally got Dr. W. H. Alloway in September, 1950.

On November 15 I gave a speech to a packed house in the Department Auditorium on The soils men live by. This seemed to have a pretty good reception and some saw that the SCS program was ^{being} pushed by Bennett for entirely the wrong reasons. *in fact it was difficult to paint out fallacies with appearing to be against actual soil conservation, which I had been teaching on for many years.*

7150

That night I left for Oklahoma City for the meeting of the Reclamation Association. I had been told to go since technical questions might arise. But none did. It was purely political and promotional. The Chamber of Commerce crowd and contractors made up the group with almost no farmers. These people wanted reclamation wholly for contracts and for the money that would be spent in their towns. Few had any interest in irrigation. Old Senator Kerr gave one of the most amusing speeches I have ever heard although it had nothing to do with reclamation. The place was simply littered with ridiculous propaganda and lies against TVA and a possible Missouri Valley Authority.

Just before I left on this trip Dr. Ignatieff had given me a huge mimeographed document supposed to be a first draft of Efficient use of fertilizers. Most of it was just terrible - far worse than I had expected. I worked on it for two days each way on the train and part of the time at Oklahoma City.

I got home about 8:00 Sunday morning November 21 and called Dr. Ignatieff. I am afraid that he had not quite finished his

morning tea! I explained to him that this was just terrible and not to let out another copy to anybody - that it was really the "Fanny Hill" of soil science.

The next morning he came out to my office with a bit less than his usual gaiety. A little after lunch he was convinced of what we needed to do and we went ahead and did it. When Ignatieff left my office that day he said, "When I come out here I never know whether I shall get a pat on the back or a kick in the pants". I wept, but not much.

Ignatieff redictated part of it and I most of it. As I recall we had a pretty respectable manuscript by the end of the week. This we sent out to many people. I also urged that he get it into French so that FAO could get Aubert to look it over. And this was lucky too because the translator didn't know much about science.

inset 352a, b, and c.

In December I was working on the Congo bulletin, the Alaska bulletin, the Food, soils, and people bulletin, and the UN paper, which was later published as Soil survey in relation to soil conservation.

11/24 - Dll

/ Sometime in the autumn of 1948 I learned that E. J. Coil, Director of the National Planning Association, was promoting a "debate", principally between Mr. Gordon Clapp of TVA and Secretary of Agriculture Brannon about the failure of the two agencies to cooperate in the Tennessee Valley. This situation had a long background history. The principal point was that when the TVA Act was passed, it gave the Board full authority for agriculture in the Valley. TVA could have duplicated all of the research and advisory work of the colleges and of the Department. At the Department's strong urging the three-way agreement was signed by the Secretary of Agriculture, the Chairman of the Board of TVA, and the President of each of the land-grant colleges in which it was agreed that TVA and the Department would conduct all their agricultural work in the Valley in cooperation with the colleges. TVA held to this. The Department did not and yet it would not abrogate the agreement.

Gordon Clapp wrote to me and asked my advice about whether or not he should accept the invitation. I looked into the matter and found that it was going to be a packed audience with loaded

questions without any thought of an objective discussion. I wrote to Dr. Black and he agreed fully with my opinion so I advised Gordon not to accept unless the discussion were in the Valley rather than the Cosmos Club. I agreed that I would go to the show and report to him.

The affair was held in the Cosmos Club on the evening of November 30. E. J. Coil served as chairman. He first introduced David Cushman Coyle who read extracts from letters from Morris L. Cooke. Coyle said that Mr. Clapp's letters were too long to read. He also quoted from other letters.

Senator-elect Kefauver spoke briefly about TVA and unhappily had to leave.

The audience was pretty well packed with SCS boys and farmers they had selected. A Mr. Hoff, Secretary of the Brandywine Valley Association, told of the wonderful service from SCS and the very poor service from Extension. He asserted that he knew nothing about TVA nor had he even read the Act.

Then a Dewey Anderson read a letter from Lowdermilk, who, he said, "had detailed and intimate familiarity with all phases of TVA".

What a farce! Of course he thought SCS was better.

Secretary Brannon started out with a strong statement of his great friendship for TVA. (If he had any it was long before I knew him. This was one of his blind spots.) Unhappily, Brannon got red in the face and quite irresponsible near the end. He was highly complimented by Charles Brand, who had tried to get me fired over the fertilizer question. I never heard more ridiculous statements with the chairman, Coil, actually trying to get votes on loaded questions.

By this time all but the actors and the guests of SCS were beginning to laugh a bit. Then an unidentified farmer from the Tennessee Valley got up and asked the audience, "How many people here live in the Tennessee Valley? - Raise your hands." Not a hand went up. He said, "That's what I suspected. I can't tell you how absolutely ridiculous this whole thing would sound to anybody living in the Valley." Dr. Bushrod Allin was there and just threw back his head and roared with laughter, which touched off everyone else except the actors and the "guests". All the chairman could do was throw up his hands. The whole farce was over.

Already arrangements had been made for me to leave directly after New Years for visits to Australia and New Zealand. Then came all the papers for the special issue of "Soil Science". The papers ran over the allotted space so I called Firman Bear and told him my predicament. I got out of the trouble by getting his agreement that I could mark passages for fine prints, ^{thus} ~~so that~~ my total editing consisted mainly of marking enough for fine print to get within the 120 pages allowed. Of course, I had seen most of the papers before in first or second draft.

This month I also had some serious administrative problems about budgets since the Bureau of the Budget was insisting that we absorb the funds required for the mandatory pay-act costs. (Later they relaxed this a bit.)

Aubert had had to be careful about money and had spent much of his valuable time giving lectures. Early in December he had arranged for a final lecture, in French, in Quebec. ^{At that time,} ~~Then,~~ when his trip was essentially finished, ^{down} his Embassy called him ~~soon~~ and gave him sixteen

fifty-dollar bills! He came back to our house and was so excited.

He peeled off several ^{little} and said, "Here is what you advanced me".

"But", I said, " You gave me one too many."

He recounted, laughed, and said, "So I did".

It was a shame he had not had the money earlier. He bought some ~~things for his laboratory~~ books, a new suit of clothes, and some things for his laboratory. He left about one-half of it with Lucille for future use.

It turned out later that this invitation to visit the United States, helped Aubert in France and promotion came soon. He was a good student and highly industrious. Rapidly he rose to be the most prominent soil scientist in ^{his} the country.

December 21 I made a speech in New York at the famous Cooper Union on Soil and world food problems. This was an interesting place right down in the ^{heart of the} Bowery. A part of the audience was in tuxedo and evening dresses and a part were Bowery bums with their bundles wrapped in newspapers who had come in to get out of the cold. Osborn and some of his crowd were there. After two or three attacks in the

in the question period they kept quiet and I had a very good time.

I had a couple of days in Knoxville after Christmas on fertilizer policy within the TVA. I got back in time to get the special issue on Soil classification on its way. This was published the following February and served for years as the authoritative supplement to the 1938 Yearbook, Soils and Men.

1949. Shortly after New Years I started the journey to New Zealand and Australia. The Seventh Pacific Science Congress was to be held in New Zealand so I arranged to go to Australia first.

In order to be certain, so I thought, of getting to the West Coast for the plane overseas, I took the train. But when I got to Chicago I learned that all trains to the west were snowbound! I took a cab to the Pan American office and they got me a seat on TWA. This left a bit after midnight January 6 and we were down in San Francisco a bit after breakfast. I took a little rest and went out to the Soil Survey office in Berkeley where I spent most of the day dictating a draft of Chapter three of Food, Soil, and People.

Detailed notes on this journey, with illustrations, are included in the two volumes of the Down-Under Journal, 1949, Vol. I, Australia and Vol. II, New Zealand.

I arrived in Hawaii January 9 and Dr. Auchter and Dr. Baver showed me some of their research and we had a little party at the Country Club.

I left Hawaii for one of the two or three most tiring trips I ever had. We stopped at Canton Island, Fiji, and New Caledonia.

The plane landed at the Sydney airport about 6:20^{p.m.}, January 11 (a day was lost). Almost immediately I found a press group waiting. I was too tired to keep notes and I have no idea now what we talked about. Throughout the trip my back and especially my neck was extremely painful.

The next day I had to get money and an ~~Exit visa~~ swearing that I had made no money in Australia. Immediately after breakfast a man came in from the local wireless station with a copy of "Successful Farming" for November under his arm and just about forced me to make a platter on the world food problem.

I enjoyed the trip to Queensland very much, especially the visit to Cairns and the Atherton Plateau. Once I got out into the sugarcane fields my back loosened up; it was like oiling an old lawnmower.

I reached Adelaide in South Australia January 20 and was delighted to see my old friend, Professor G. W. Robinson. He was going to New Zealand the other way around. I was pleased with my trips with Stepnens but J. K. Taylor and Prescott seemed to be spiritless.

Stephens had arranged a wireless talk and when I finished the man handed me a check for a few pounds. I explained that I couldn't take it because I had sworn to his income-tax people that I wouldn't earn any money. Stephens insisted that I turn it over to him and that he would buy me a little library on Australia. It came a few months later and many of the books were very interesting.

I had a rough trip to Perth with evening travel both going and coming. I stopped for a few moments in Adelaide on the way to Mildura on January 27. The boys took me immediately into the field. About 5:00 p.m. I said I wasn't understanding a thing and they took me to my hotel. The next morning I was up very early from the screeching of a multitude of little lovebirds just outside my window. I had a good go with Butler in the Murray Valley. The roads, the heat, and the dust were frightful and the little hotels worse. My back really caught it. I got back to Sydney and a nice hotel room January 30. About 3:30 the next morning when the porter brought my tea I heard a crushing sound. He left immediately. He had put the platter over my glasses and broken them. When I got my spares out I discovered

one of those was also broken. Thus I had to patch my glasses with scotch tape during the rest of the journey.

Prof. Robinson and I left Sydney in an old flying boat about 6:00 in the morning. We expected to arrive a day in advance of the Congress and get a good rest.

We got down in Auckland ~~xxxx~~ about 4:00 p.m. and were met by our New Zealand hosts, including Dr. Grange, then Chief of the Bureau of Soils, whom I had met the previous year in Rothamsted.

Curiously, no matter how hot, Grange always wore underdrawers held up ^{with} white loops where his galluses buttoned on his outside trousers.

I had hardly caught my breath before he said, "There is nothing on tomorrow. The next day is the formal opening and you won't be interested in that. So I've arranged a trip to North Auckland where you can see beautiful examples of the Kauri Podzol. Norman Taylor and Sutherland will be at the most northern part with a car and we can go up on a plane. There is only one difficulty that you won't mind: We shall have to start very early in the morning". No rest!

from Feb

Yet it was worth it. We had a wonderful trip. Nearly everything was new and I asked for a lot of stops. It began to be late on the second day. I remarked half aloud, "I don't see why I'm always out so late." Norman Taylor said, "I could tell you but I won't".

We had some wonderful field trips in New Zealand with beautiful scenery, interesting soils, and delightful companions. Prof. Robinson, Prof. Kay (Reading, UK), and I, along with either Grange or Taylor went together a good deal. On one occasion the two professors went to sleep in the bus. I went forward cautiously and got a good photograph of them. A couple of months afterward I sent one to each, with appropriate captions. Prof. Robinson was not amused. Norman Taylor has recalled many times one remark he said I made to him: "I could have made a soil map of New Zealand much easier if I had not come here".

Just after he arrived, Holt, one of Bennett's boys, talked with the press about the terrible erosion in New Zealand. With most of the land in grass!

File

The Congress finished at Christchurch. By special arrangement, two of the boys took me on an interesting field trip. Among other things I saw loess in formation with beautiful clouds of it coming out of the braided beds of the glacial streams and settling on the uplands. My pictures of this phenomenon were essentially duplicates of loess formation in the Matanuska Valley in Alaska.

I left Christchurch on February 23 and reached Hawaii on the 24th (of which I had two). I got to see a little more of the research and left the evening of the 25th. I got home early in morning of the 27th of February with some sleep to catch up.

I caught up on my office work and started to dictate a journal of my trip. At this time I had two secretaries, one for administrative work and one primarily for professional work, who was a veritable jewel of a secretary - Dotty ~~Barroughs~~. (*Dorothy M. Barel*).

I went to Minneapolis on March 9. (Insert 361a and 361b)

1949

on March 11

By previous arrangement I had agreed to speak at a combined business and farmer forum in Minneapolis on "Soil Conservation and World Food Needs." This turned out to be a very funny show. One of the big shots in the National Association of Soil Conservation Districts came, Kent Leavitt. When I got there Iver Nygard told me that Ralph Musser and other SCS boys had been there for two or three days planting questions with Soil Conservation District Supervisors who were meeting in Minneapolis at the same time. I can't recall the name of the very nice farmer from Southern Minnesota who acted as my personal host. He said he was an ordinary farmer but in my conversations with him it came out that he judged cattle all the way from Chicago to Kansas City to Ft. Worth, had his own airplane, had an elevator in his home.

I recall giving the lecture at a noon meeting just after lunch. The crowd was large and attentive. After the lecture during a short intermission the hotel staff removed the tables and brought in chairs. About 2 P.M. we started a panel discussion of my luncheon address. Dean Walster was chairman. Besides myself on the panel were Kent Leavitt, a planned-parent-hood advocate and a confused business man who was scared but did not know quite what he was scared of.

Kent Leavitt attacked me in the field of soil science and it wasn't hard to turn all of his examples wrong-side out. The madder he got the more irresponsible he became. He accused me of writing an article in "Time Magazine", which I didn't, but I had talked with the reporter and told him if he brought my name in ~~a mile of that~~ ^{to} piece I would haunt him the rest of my life. So he talked with Dr. Salter and used his name ~~in the picture~~ ^{and}. So all I had to do was to tell Kent Leavitt to go back and read it. The audience began to be amused. Iver Nygard sat near the front and laughed so hard that tears were running down his cheeks. ^{pleading} About that time I recall Kent Leavitt saying in a ~~blatant~~ tone, "We farmers can't understand how to handle modern technical farming. We have got to have these smart boys from the colleges come out and show us how to do it." Some of the ~~the~~ Swedish farmers looked up in astonishment. I winked at one prominent looking one and he winked back and smiled. (Kent Leavitt had married a farm. His wife had a herd of Brown Swedish cattle and sold the products to a New York night club at fancy prices. She did not let him have anything to do with this enterprise. Actually he was some kind of bond salesman posing as a farmer.)

Then came some of the planted questions. The people that read these did not know what the words meant let alone the questions. Things got funnier and funnier. Finally, some man stumbled through one of these questions, which I can't remember, but ended it up with this. "Can we afford to be as complacent about these problems as you apparently are, Doctor?"

As chairman, Dean Walster said, "I have known Dr. Kellogg for a long time and I have heard him called many things. But this is the first time I have ever heard anybody call him complacent about anything." The audience simply roared. The Dean threw up his hands and said, "The seminar is concluded."

Several people came up to congratulate me including the old Swedish farmer that winked at me. He asked me, "When does your train go back to Washington?" I replied, "About 11:30 P.M." He said, "Fine, we are having our annual banquet of the Minnesota Soil Conservation District Supervisors. We should like to have you be our guest of honor." So I went. This was a terrible blow to Kent Leavitt and he never spoke to me again as I recall.

March

I met Dr. Black in Chicago and we went together for talks with Charles Hardin.

From March 20 to 30 we had the Work-Planning Conference of the National Cooperative Soil Survey in Memphis. I explained to the staff that I must have all committee reports by autumn and I discussed with them the outline for the revised edition of the Soil Survey Manual. Already several had been asked to recast their committee reports so that they would fit in fairly well as sections of the Manual.

From Memphis I went over to Chattanooga and gave a speech to a large meeting sponsored by the Department's Production and Marketing Administration about the potential^v of soil resources in the United States in relation to farm planning. It went over very well indeed. But of course the PMA was glad to hear anything besides Bennett's stuff.

In April I had the Food, Soil, and People booklet just about finished. Although UNESCO didn't like the idea much, I insisted that this had to be cooperative with FAO.

Beginning especially in 1949 the Soil Survey had a great many foreign visitors: Both Fripiat and d'Horre from INEAC, J. L. Burnotte

from the Kivu in the Congo, Saunders from the UK, Raeside from New Zealand, and others. A little later I arranged trips for Alex Muir, Tom Walsh, Sir William Ogg, and C. H. Edelman.

In the middle of April I took a short trip out to Ames to talk with President Friley of Iowa State College and some of the Board members about the open position of Dean of Agriculture. Both Dr. Pierre and Dr. Kildee seemed very anxious for me to come. I just could not register a lot of enthusiasm. I was not offered the job. It would have been a difficult decision if I had been.

On the way back I had a visit with Prof. Schultz in Chicago.

Early in May I gave a talk as part of a symposium on renewable resources sponsored by the United States Chamber of Commerce. They published my talk as Prospects of Soil Science. Until I got there I didn't realize that Fairfield Osborn was also on the panel, otherwise I should have refused to come. Louis Bromfield also spoke but by this time he had left the Bennett camp. Earlier, out of a clear sky, Bromfield had written me a personal letter apologizing for some of the nonsense he had written. Fortunately Osborn was scheduled to

speaking before me. He had a manuscript in his hand. He kept looking at it and then around the room; but when he got up to speak he left it in his chair. No doubt he had some of his usual nonsense in it and didn't want to run any risk. When he spoke he sort of rambled around without much to offer. My talk seemed to go over very well and we had a good question period. Some of the SCS boys were in the audience but they didn't say anything.

What with garden soils, the world food problem, and farm planning I made many local talks in the eastern states and in the District of Columbia.

As I look over the correspondence of the last several years I realize that Lucille, Robert, Mary Alice, and I spent a lot of time in the garden. We had such poor soil that most of it had to be reworked for fine plants. We took out hundreds of bushels and brought in good woods soil, first from a large vacant and wooded lot nearby and later from the lot we had bought as an investment. We tried a great many things but got the most satisfaction from azaleas and a few others. In 1949 we actually won a prize for having made

the most improvement of lawn and garden in Hyattsville. Over the years we must have set 30 tons, or more, of stone in little terraces to make the water soak in. In spare moments I began to outline a book on soils for the home gardens using hand tools.

In May Alex Muir came and staid with us part of the time. I was much amused one morning when he came down wearing shorts ready for a field trip to New York in late May. I looked at him and was about to explain the hazards of insects but I didn't get a chance. He asked, "This is a free country isn't it?" I could only reply, "It certainly is. Anyone can wear what they like and backwards if they want to." The boys told me later that poor Sandy's legs were just covered with mosquitoes but he never said a word. Also he didn't wear his shorts again.

On the 21st of May I went to Louisville and gave a talk on Food, Soil, and People, based on the UNESCO manuscript which was essentially completed. When I went to leave someone had put a quart of bourbon whiskey and a box of whiskey-filled chocolates in my case so I just closed it and brought them home.

May 24 I went to Harvard for one of Dr. Black's many seminars.

After the land-use seminar Dr. Black and I were talking in his office.

He pulled out his watch, looked at it, and said, "The general economics

seminar meets at 4:00, ten minutes from now, and you are scheduled

to speak to them on the world food problem." This is the way the

old boy did. I rushed into ¹⁶another office and used the ten minutes

to work up a little outline. I hadn't realized until then the

enormous size of the staff of the Economics Department, including

graduate students. I had a good audience and a nice question period.

As I recall DeVoto's wife was there. She might have been all right

but I couldn't endure her husband's writing about soil resources.

I had been asked to go with McCall and Lambert as a team to advise the British Colonial Office about agricultural problems in

Africa. But it was just out of the question and I was able to get

Cline to go in my place. He was very grateful for the experience

and got to love Dr. McCall as we all did. They both told me separately

that Lambert was so arrogant and officious that he nearly spoiled the

trip for them and created a bad impression of American deans of agriculture.

366a net

About June 8 I gave a speech on soil resources and farm planning to a lively meeting in Savanna, Georgia, of the FMA for the southeastern states. I recall one old boy coming up to me and saying, "That was a fine speech. I sure wish 'Ole Huhman' was here. He woulda liked it."

June

Also in June Dr. Roy W. Simonson came into my office as Assistant Chief of the Division. After Baldwin had retired Ableiter took over the position of Chief Soil Correlator. He was a bit slow and I had to have someone else if I were to continue with my many other interests.

One day in June someone called from the Mutual Security Administration about a visit of an Irish soil scientist to this country. They read over the itinerary and asked me about it. It was just terrible for a soil scientist. My caller said, "That's what I thought. Suppose we cancel it and I have the boys ^{meet} ~~met~~ him and bring him out to your place for a new one." I agreed. In a day or two Tom Walsh came in.

Tom was a big, strapping, black-haired Irishman toward whom I took an immediate fancy. After we had fixed up the itinerary, I suggested that he come out to my home on Sunday. He came about 10:00 in the morning. He said he could stay only a short time, but ended by staying all day. We had a most delightful talk about what could be done in Ireland. As it turned out I probably never spent one day more usefully.

Tom has often spoken to me of that day. *All arranged in
excellent manner again in 1952 by the same*

A little after the middle of June I went out to Ames for a seminar in land economics. I spoke to the soils class on Alaska and gave an evening lecture on Farm resources of the United States. On the way back I had a conference with Dr. Peirre in Chicago, who was temporarily guest lecturer at the University.

I don't recall when, whether in June or a little later, Efficient use of fertilizer was published. Nearly all of the reviews were extremely good except one. This one was by a Mr. Brown, Bennett's agronomist. He was kind to the text, which was anonymous but complained bitterly about the photographs, most of which were mine. Actually the photographs were quite good and the FAO boys got a bit upset until I explained that was the normal price they would need to pay for associating with me. One wonders why people do such things.

July 5 Lucille and I started for Michigan. The next day she left me at Ann Arbor at the university facility I was to stay in as a guest. When I walked upstairs I suddenly realized that my ankles were extremely sore. When I took off my shoes I noticed that the ankles were badly swollen. I spent the afternoon reading and writing

and went to bed early. The next afternoon I gave an address on Soil and food. The group was not a large one but very good. Several had come down from East Lansing for it and I rode up with them. On the 8th I had conferences with several people about the cooperative work and again went over in detail the status of the several unpublished Michigan soil surveys. Most of these were done by or under the direction of Prof. J. O. Veatch - what he called "land-type" surveys. The base maps were poor and so were the notes. J. O. and his assistants took very few notes. Any land type was what J. O. said it was when he looked at it. A few were soil types, some were phases of soil types, and most were undefined complexes or associations. Had the work been done consistently and the mapping units described those with good base maps could have been published but most were just a mess. Dr. Nygard spent quite a ~~lot~~ of time trying to figure these out but it was nearly useless.

Lucille came to East Lansing July 9, and we had dinner with friends, then drove to the Kellogg home in Ionia County. We staid for about three days, returned to Hyattsville and then took a holiday for a week at Crestmont Inn near Eagles Mere, Pennsylvania in the Poconos.

My ankles were still swollen and sore. I started to work there on a paper to be given at Amsterdam on Tropical soils.. I was also putting the finishing touches on a large paper about the soils of the Belgian Congo to be published in three parts in "Soil Science".

Shortly after returning home I discussed this problem of my ankles with the physician. He didn't know the cause but felt that it was unrelated to the arthritis in my back, which was also quite painful during that summer. Near the end of July I got my payment for the UNESCO bulletin. (These people were not good to deal with. They had promised me that any translations or other editions would be shown to me in advance and translations checked by a person of my choice. They did not do this and the British edition was terribly mangled. The only translation that was checked by a competent person was the Icelandic one. And yet all of these agreements were written into the contract.)

Early in August I had some talks with the people handling the Agricultural Conservation Program in PMA about farm planning along the lines that Dr. Black, Sherman Johnson, and I were thinking. But

we made no significant progress.

August 17 I went to New York for the long awaited United Nations Scientific Conference on the Conservation and Utilization of Resources (UNSCUR) at Lake Success. A great many of us staid at the Barbizon Plaza Hotel. I worked out a scheme for Ogg and Edelman to leave a bit early, travel to Iowa, and then be taken by Guy Smith to a conference Truog was handling on the mineral nutrition of plants. Clifford Orvedal and I had prepared a paper for it and he presented it.

The UNSCUR meeting was a bit leisurely with the usual plenary and specialized sessions. On the whole it was well organized, much better than the one in Geneva in 1963. I recall that at an early plenary session Colin Clark made a terrific boner by predicting that farm labor would increase relative to city labor. I simply couldn't let this pass and challenged him on it. To this writing I could never see how this man could keep a professorial position or get any large part of his papers reproduced. He must have some secret that few others know.

Many of the papers were quite good and the discussions were handled very well indeed. I had some good arguments with my friend Sir Geoffrey Clay of the British Colonial Office and many others.

A. B. Lewis gave his discredited paper again. It was long, scarcely audible, and very dry.

A delegate from Venezuela told me seriously that his government had directed him to get Bonnet (Puerto Rico), Vogt, and Kellogg to come down to advise them for a week!!

On Saturday afternoon (August 20) I took a long walk down Fifth Avenue hoping to find some bookstores open but none ^{was} ~~were~~. On the way back I came upon Secretary Brannon and his wife out window shopping. We stopped for quite a long talk. Mrs. Brannon had a remarkable memory and always asked about my arthritis. (At that moment it was quite bad.) The Secretary spoke ⁱⁿ very uncomplimentary terms about a speech Bennett had ~~g~~iven at the Conference. I said, "Perhaps you know most of that was intended to discredit my own work." "Yes", he replied, "I know that. The man is terrible. Does he really know anything at all about soils?" I told him, "Bennett knows something ^{a bit}

about erosion but he is unable to relate that to other aspects of soil management and practical ~~ag~~^{ag}riculture. Farmers don't farm to control erosion. In fact, soil erosion is more a symptom of distress than the cause - an exploited people pass ~~their~~^{their} troubles on to the soil."

"I'm sick and tired of Bennett and his SCS" Brannon said. "But you can see my position I hope. He has only a couple of years to go and I can't fire him now".

I went to a play - "A Streetcar Named Desire". It was a bit rough.

On August 23 I gave my paper, Soil survey in relation to soil conservation. It seemed to go quite well. So far as I know during ~~many years~~ many years this remained the authoritative guide for initiating soil surveys in underdeveloped countries. To my amazement I was told that even Hugh Bennett spoke well of it.

On the 25th I presented the paper on The corridor system of shifting cultivation written by Jurion and Henry of the INEAC. I had to defend this paper against absurd attacks by the French.

I attended nearly all the sessions and had conferences and many dinners with my European friends. I remember one session in particular

with a gentleman from Syria in the chair. A delegate from New Zealand had advocated brush burning to prepare land for pasture. A delegate from some country in West Africa took violent exception and said that burning brush would cause only erosion and not help the pasture.

The chairman looked down at the end of the table at me and said, "I see Dr. Kellogg here. Sir, can you explain why this conflict?"

I replied, "I think I can if I may ask the speakers a few questions." The chairman nodded approval so I asked the delegate from New Zealand, "Don't you always use superphosphate immediately after burning the brush?"

"Certainly we do," he replied, "otherwise we wouldn't get any grass".

Then I looked at the delegate from Africa, "Do you apply fertilizer after burning?"

"Of course we don't", he said, "It is far too expensive".

I addressed the chairman, "There you are, Burning brush in one system of management has an entirely different effect than it does in another system".

By the next Sunday, August 28, my back and neck were again very painful. The following day I went to the Education Section, which was pretty bad.

I wrote Robert a long letter for him to have on the occasion of his twenty-first birthday.

The Conference ended very pleasantly on August 31. In the evening the British put on a nice party and the next day I returned home.

Early in September the paper on the soils of the Congo was finished for "Soil Science". I sent it over to Jurion for review and criticism. He wrote immediately that he would very much like to have this included in the INEAC scientific series. Since INEAC had paid all my travel I agreed and made my peace with Firman Bear. *I had experienced* This turned out to be the most rapid printing, and also a better job than any by our Government Printer. Jurion sent the manuscript back to me for a few corrections. Then I returned it to him and he sent the galley proof to me. This I returned to Brussels and soon had the page proof. Of course this had to go across the Atlantic again. Early in December I had a few copies of the finished bulletin. An exploratory study of soil groups in the Belgian Congo. Miss F. D. Duvall went along as junior author since she had done most of the laboratory work without knowing one thing about tropical soils.

Also in September I looked over several candidates for the position of leadership in soil survey interpretation left by Dr. Barnes. I had hoped to have an economist but I was unable to find one interested in the work with enough training in basic science to understand and

to evaluate the available data.

Sandy Muir left to return to Britain about September 14. He wanted to leave his excess dollars with us so that we might send^d him books. Just as he left New York City for the airport he bought two postal money orders for a total of about \$130. The money orders came just as we were leaving the house. I opened the letter saw what ~~it~~ was inside, laid it down somewhere. I think it was the next day that I left on a trip and wrote Sandy that I had received the money. When I returned I asked Lucille about the money orders and we simply couldn't find them anywhere. I wrote Sandy and asked him to tell me every detail he could remember. He had thrown away the receipts but happily remembered the exact postal station where he bought them and the time of day. So I wrote to the postmaster at that station and explained what had happened. Luckily he believed me and sent us the money.

Although I liked Sandy very much I could see that he was going to fail with the British Soil Survey. He and Ogg took the extreme position that the soil survey was wholly a scientific affair and

took no responsibility whatever for any kind of interpretation.

His published soil surveys were highly technical reports on soil morphology and genesis. They contained essentially nothing for farmers, foresters, gardeners, or engineers. My plan for his visit in the United States included some of our best men in interpretation but it didn't do any good. What a wonderful opportunity was missed.

About September 18 I joined Director Lewis of the Texas State Experiment Station and others to look over a tract of land near Waco that was being turned over from war use to the Station. Director Lewis had been receptive to the idea of pilot-research farms. I encouraged him and was anxious to see the plan given a good trial. But his staff was chilly to lukewarm and finally nothing worthwhile was attempted.

I went on from Waco to Riverside, California for conferences at the Salinity Laboratory. Dr. Hayward was ill and L. A. Richards was acting for him. They wanted one of our soil scientists stationed there to work with them on the practical aspects of interpreting data in irrigation project areas. They had got a bit crossed up

with Ray Roberts. I told Richards frankly that they had unfairly expected Ray to know as much about the tiny sector of soil science they were working in as they did. Ray had a much broader area to cover. He sensed their attitude, and simply kept quiet. I agreed to send them W. G. Harper provided they were willing to accept him on an equal basis. I said to Richards, "If you boys get uppity with Harper he'll tear the place down. So make up your mind." Harper came later and the relationships were excellent right up to his retirement. Actually, within the first two weeks after his arrival he saved them great embarrassment by getting an originally favorable report of an area reversed before final release.

At the end of the first week in October I attended a sort of "far out" conference of the "Post War World Council" which was held at the headquarters of the American Philosophical Society of Philadelphia. The interest was stimulated by the Point Four program of the Department of State. Among those present were Stringfellow Barr, Seymour Harris, Harlow Shapley, Norman Thomas, James P. Warburg, and the like. The Department had hesitated a bit about whether I should go.

The conference was perfectly harmless but way up in the air. It was very difficult to get their attention for the elementary details of agricultural development, which is essential to economic development in the poor countries. ^{They} ~~These~~ spoke the vague language of UNESCO.

Now I was pushing pretty hard on the Alaska bulletin and several speeches.

The Soil Science Society meetings were held in Milwaukee October 24-28. That year the Society put on a special program commemorating the 50th anniversary of the Soil Survey. We spent quite a bit of time on a draft constitution for the International Society to be reconstituted the next summer. I talked about inviting the Congress to the United States in 1954 and the Society was willing that this be done. (Later the Department of State also approved but I was doubtful that our European ^{friends} ~~friends~~ would have the money to come.) As usual I spent much of my time in private conferences about the cooperative work. I gave a paper on the special program - Future of the Soil Survey. Just before leaving I had another conference with J. O. Veatch about the old Michigan soil surveys. He promised

six of them by Christmas but, of course, they didn't come. He lead his bosses at Michigan State and me down the garden path on these. He wanted to stay on the payroll. To do that he had to promise to get these surveys done but I am sure he had no intention of doing so.

October 29 I took the bus to Madison. I talked with ^{back} the Dean and Muckinhirn and tried to dissuade Muckinhirn from going into the Dean's office. For some time Truog had planned on him as his successor as Head of the Department of Soils. In fact Truog was very much irritated with Muckinhirn and I was too. Possibly Muckinhirn ^{felt} ~~felt~~ that he didn't have the physique. Perhaps he was over anxious to be an administrator. But he went to the Dean's office and over the years became more and more provincial. It was such a pity. In the beginning he showed a great deal of talent but he lacked sustained ambition or something to develop his talents.

October 31 I left for the Mayo Clinic where I stayed until November 6. I spent most of my time waiting in line. The first day I was in the clinic from 7:00 a.m. to 5:00 p.m. with about thirty minutes of medical consultation. I asked to have my glasses

adjusted and they broke them so again I had to wire home for new lens. The brace shop was to fix up a new brace but somehow they couldn't get at it for two days. In fact this time I thoroughly hated the place before I left and I paid \$120 for the priveleges of waiting in line.

Sometime in November J. W. McKericher retired. I don't think he really wanted to but Allanson talked him into it and I certainly encouraged him. Mac had been of enormous help to me up until 1945 and after that he deteriorated so much that I couldn't depend on him at all.

From time to time I began to realize that the FBI, USDA, and Civil Service investigators were looking into my "security". President Truman had begun this kind of thing in the wrong way and then a House Committee under various irresponsible chairmen and a Senate Committee under McCarthy, carried this business to great extremes. The more ardent leaders, including J. Parnell Thomas, Nixon, and McCarthy were using this method to harp~~ass~~ the intellectuals. I can't recall now of a single Communist who was exposed although

several came to the Committee, confessed their Communist connections, and were cleansed. This was a terrible period, especially for members of the foreign service overseas who reported the truth, which ^{often} was not consistent with the prejudices of the extreme right.

I never took this business too seriously and can't recall the order of all the events. Henry Allanson told me in the summer of 1949 that the clearance for my passport was very slow in coming for the trip to New Zealand. I don't know how many times I explained to various investigators the circumstances under which I was ordered to make the speech in New York City in 1943 that was sponsored by the Science Committee of the American-Soviet Friendship Society.

This whole investigation procedure was doubly ridiculous because I was obviously accepted by three other principal agencies to help them with problems of a far more sensitive nature than anything dealt with by the Department of Agriculture or the agricultural program of the Mutual Security Administration. The congressional committees called up Harlow Shapley and some of the others of us who had gone to Moscow in 1945, but fortunately not me. This extremism made a

lot of people talk in whispers and made recruitment difficult.

At the worst time only a small percentage of graduates from Harvard and some of the other good schools would consider a position in the government.

A large factor in the situation was the enormous rise of science, especially in the military program. Politicians didn't understand it and basically resented the fact that they had to depend on scientists. The heart of the problem was best portrayed in a cartoon by Herblock. It showed J. Parnell Thomas, Nixon and others sitting around a table with Thomas pounding the table and saying, "What I want to know is who gave those secrets to the scientists in the first place."

I never let all of this worry me too much for the obvious reason that no incriminating facts could possibly be found. Yet it was uncomfortable to know that some men equally innocent had been framed and the stories of the accusations got the front pages while those of their complete lack of guilt got the inside pages. At the same time the Soviet magazines were calling me a "tool of American imperialism".

Early in December I went to Little Rock, Arkansas and gave a speech on soil resources. Afterward I was "carried" in a car to the University of Arkansas at Fayetteville and spoke to interested students about soil science as a career. On December 9 I took two large sections of the Junior soils course and talked to each on tropical soils. That evening I went with the students to an Alpha Zeta banquet. The Assistant Dean, Dr. Hudson, took me down to Ft. Smith the next morning to get the plane east. It was late because of fog, so I gave it up and got the train to Memphis. This was a dreary ride through Arkansas. Most of the cleared land was now growing broomsedge but here and there, at about mile intervals, I saw green fields. These belonged to farmers that had somehow learned to use lime despite the University. Up till this time the so-called soils leader (Bartholomew) at the University claimed there was no need for lime in Arkansas. What a stupid fellow he was! (When he had refused to cooperate with the Agricultural Conservation Program, I agreed with them that I could go myself to Arkansas to train their people. Once the University

learned this, they began to help.) The train arrived in Memphis during a pouring rain and I went to the Peabody Hotel.

The next day I flew to Knoxville and spent two days with Moon, Bass, and the others of TVA, mainly about fertilizer policy and a revised statement of the agricultural objectives of TVA. We also had long discussions about the "special farms" within the test-demonstration group on which I hoped that at last we could get some real good records.

When I returned to Washington some of the nice blue bulletins on the Soils of the Congo were waiting for me.

Earl Fowler retired in December and arrangements were made for Dr. Guy Smith to come in early in January.

For some time Lucille and I had been making our plans for a big trip to Europe to include the Congress at Amsterdam, Ireland, Portugal, Belgium, France, Britain, and Iceland. Years ago Lucille had taken out an \$1,000 endowment life insurance policy which came due in September. This was earmarked exclusively for her use on this trip. We also realized the need for a will of some sort.

I had seen many scientists with both smaller and larger libraries than mine fail to make provisions for them so that after their deaths the collections were broken up and lost. The Department of Agriculture Library couldn't accept collections to be kept together, so I was told at least. Cornell seemed to me to be the best place because of the first-class young men already there. Also they gave no honorary degrees ^{to} ~~to~~ anyone for anything. It was about Christmas 1949 that I began negotiations with them that resulted in a will providing that Cornell would have all of my scientific, technical, and agricultural books, bulletins, notebooks, papers and maps, wherever situated, provided the collection was maintained as a unit to be available to resident and visiting scholars.

I spent a bit of time after Christmas on draft-policy memos to send to TVA, and speeches, but concentrated mainly on the draft of the Soil Survey Manual and the Alaska bulletin.

Beginning early in September I devoted all of the time I could until leaving for Europe, June 24, in developing a good first draft of the Soil Survey Manual. Without Dottie, who was a remarkable secretary, I wouldn't have made it. I dictated quite a bit of it at home evenings and weekends on discs. Dottie had a special helper to transcribe these. She often came herself on Saturdays and had other days off when I was away. About half of it was reworked from the committee reports or rough drafts by others and a/pit ^{one} ~~one~~-half from my own notes. I reworked nearly all of the rough first drafts myself in enough copies to send a fair second draft of each section to the staff members best informed on each subject. This became a pretty steady grind for many months ahead.



1950. About the turn of the year I sent my paper on Tropical soils to Edelman for advanced printing. I was a bit rushed to be sure to complete the Alaska bulletin and the first draft of the Manual before going to Europe. I loved Iver Nygard but not his procrastination about getting his material to me. Iver was brought up on the Swedish syntax and backed into every paragraph like a mule into a thorn bush. His observations were accurate but his descriptions of them had to be reworked or perhaps it would be better to say, translated. Early in the year I sent him an order that he was not to do anything else but get this material to me.

I left Washington the afternoon of January 5, reached Miami, Florida the next morning and gave a lecture on Soil for world food production to the American National Livestock Association. For the most part they were having quite a holiday with side trips to Cuba. I left directly after my speech and arrived home the next morning.

January 15 to 21 I spent in Knoxville with emphasis on the "special study" farms and nutrition. We also had a joint conference between TVA and PMA. The second evening I addressed the USDA Club.

Most of our time however was taken up with a special nutrition conference attended by Dr. Maynard and Dr. Beeson as well as many local people. I also spent a little while with Dr. William Ligon who was then principal soil correlator and having quite a bit of trouble with some of the SCS boys, especially a one-eyed man by the name of Glen Fuller. He had worked in the same area with Moon and me many years ago in Michigan. His work was very poor and the Michigan people asked the Bureau of Soils not to return him to the state. But they praised Moon. A little later he was eased out of the Soil Survey and when Bennett started his Soil Erosion Service Fuller was just about out of a job. He was extremely loyal to Bennett, who put him in charge of "erosion surveys" for a while. But he had to take him out of that. Instead of firing him Bennett gave him this job in the regional office under Buie. Apparently Buie was told to be kind to him.

Fuller had neither ability nor integrity as a soil scientist.

Somehow he had learned about the letter from Michigan and cordially

hated Joe Moon as long as he lived. He did everything he could to embarrass him. Any SCS man found cooperating with Moon knew that he would be in serious trouble. He ignored Buie and Hockensmith and went directly to Bennett. He would sign agreements in the field and see that they were violated. When Dr. Simonson became principal soil correlator for the southern states he transferred his hatred of Moon to Simonson. And the same thing happened to Dr. Ligon when he took over from Simonson. Of course, actually Fuller was a weak man without principle or ambition doing what he knew Bennett would enjoy. This placed the good state soil scientists that SCS had in a very bad position. Fortunately or unfortunatelly, depending on one's point of view, there weren't many good ones in the south at the time, perhaps three or four.

During the remainder of January I concentrated on the Manual and the Alaska bulletin. Some arrangements did have to be made for the Amsterdam Congress. The Department recommended and the State Department appointed a 17-man official delegation with myself as chairman and Bradfield of Cornell as vice chairman. Hugh Bennett,

Emil Truog, and W. H. Pierre were appointed but failed to attend.

In February a bad situation developed in Kentucky. The SCS had somehow sold the Governor's office the idea that their "soil conservation surveys" could be used to guide rural tax assessments, which was ridiculous. Their surveys at that time dealt mainly with erosion hazard and the mapping units were in no way homogeneous in potential productivity. The University of Kentucky had not been sufficiently aggressive in the work and got quite well squeezed out. Happily, the so-called research director of the State Board sponsoring the appraisal program came to see me. I explained the principles involved in land appraisal and why the SCS maps would be practically useless as objectively as I knew how. This shook him quite a bit. Gradually the scheme died.

At the request of the "Scientific American" I wrote for them an article on Soil which they featured in their July issue for 1950. This seemed to go well and they were still selling reprints of it for many years.

Mostly in February Dottie and I were "bashing it out", to use Dr. Ignatieff's favorite expression, on the Alaska bulletin and the Manual.

The last day of February I met with Dr. Black's land-use seminar at Harvard and then took part in another seminar dealing with the problems of the North Carolina Piedmont. After a group dinner at the Faculty Club several of us went back to the seminar for a session on program coordination and centralization versus decentralization.

The next day I had more talks with Dr. Black about agricultural adjustment and then left for Concord, New Hampshire to talk about soil tests at a meeting of the ACP folks. Many of them tried to get me to say that there were not really necessary for guiding lime applications, which I would not do. The next morning I returned to Cambridge, spent the afternoon and evening with Dr. Black and some of his students, and took the night train for Washington.

Dean Longwell of Missouri had urged me to come there and make a speech at their farmer's week for my expenses and an honorarium.

Since I had to be in St. Louis anyway for the Work-Planning Conference of the National Cooperative Soil Survey, I went a bit early and got to Columbia the evening of March 8. A whole series of conferences had been arranged. In the forenoon I gave my speech on Our soil resources: Their use and limitations. Albrecht was then Head of the Department of Soils and had been furiously writing a great deal of nonsense about the direct effects ^{of} ~~to~~ liming on food quality. I recall commenting on this in the speech and pointing out that if a diet were made up from crops grown on the best soils in Missouri and a comparable one on the poorest soil, the first would be somewhat more nutritious. But by adding or subtracting a quart of milk a day the small difference would be overwhelmed. Prof. M. F. Miller and the others were highly amused. It is very difficult to understand how M. F. Miller, whom everyone respected, and the others at the University let this quack stay on as Head of the Department of Soils. He wrote bulletins and hundreds of papers for all sorts of curious publications based on 14 animal experiments. He gave no idea of his

experimental errors but by giving him the benefit of the doubt the men at Cornell Laboratory analyzed them according to the experimental errors of the best experts with animals. Only one had statistical significance. That one was repeated by the Laboratory in cooperation with the North Carolina Experiment Station with opposite results.

We had a good session at lunch but the afternoon program could well have been shorter and funnier.

In the evening we had a conference on "Balanced farming" which was Missouri's alternative to the soil conservation districts run by the SCS.

The next day, March 10, I was shown around the laboratories and the long-time experimental plots at "Sanford Field." At luncheon I discovered I was to talk to the staff about Australia and New Zealand and then in the afternoon to give a lecture on tropical soils.

The next day, March 11, Dr. D. D. Brown of the Economics Department took me down to Osage County to see farms in a "balanced farming ring". My back was just killing me but I had a good supply

of aspirin.) Two of the local extension people were out with us.

I had some fun with the boys. They told me, "Now you just go ahead and ask these farmers whatever you want." It was raining most of the time so the interviews were in the homes and looking out the windows. I explained one or two mistakes in the first farm plan and the farmer agreed. But on the whole his books looked good. Finally I asked him, "Did the ACP payments help?"

"Help", he exclaimed, "I couldn't have done a thing without them". The boys didn't like this one.

At the next farm we had a similar experience. I asked this farmer, "Who is your local committeeman?" With a little prompting from his wife he could tell me. Then I asked him, "Did you vote for him in the last election?"

"No", he replied, "I don't bother to vote in these committee elections".

I scolded him for this, and said, "Both the University and the Department of Agriculture insist that these are democratic programs.

How can they be if men like you don't even bother to vote?" Before we left he promised me that he would vote in the next election.

Some aspects of this program were better than the SCS program and some were not so good. The field layouts were poorer. In fact "Balanced Farming" wasn't a very good club that Missouri had to beat SCS over the head. (This fight between SCS and the University continued for nearly 15 years more.)

The next day was Sunday and Shrader drove me to St. Louis for the Soil Survey Work-Planning Conference.

(Instead of giving one speech I gave several at the University of Missouri, all on annual leave time. Not a word was said about expenses. So later I wrote to Dean Longwell and sent him a list of them with a reference to his letter to me. After a long delay he sent me the expenses but not an honorarium. Needless to say there were no return engagements.)

The Work-Planning Conference of the National Cooperative Soil Survey began March 13 and ran until the 18th.

First of all, I had some problems that I had run into in developing the draft of the Soil Survey Manual that I wanted the committees to work on; and we spent part of the time on emerging problems of soil classification. I already had in the back of my mind that once we got the Manual out of the way we should make a serious re-examination of the whole system of soil classification. Actually we had two systems - one for the great soil groups and one for the soil series. The two had never really been meshed together.

I also spent a little time, mostly wasted, reviewing House Bill 6900 which proposed a broad comprehensive survey of our land resources, beginning with the primary control network, then topographic surveys, and finally the basic resource surveys with special emphasis on soil surveys and geological surveys. (The support for this bill became confused partly because of the muddying of the water by the SCS "soil conservation surveys," and it failed of passage.)

Near the end of the first day the discussion turned toward the re-interpretation of the best of the old soil surveys where the maps were good. Not many people have wanted to discuss this important problem. We did not have money in the Soil Survey to republish the texts and the State Experiment Stations would not go to the trouble of preparing small updating pamphlets.

Professor Krusokopf of Missouri was there. He was always complaining. In one mood he insisted that the maps needed to be more useful and in another mood that the Soil Survey was a scientific activity that should not be concerned with such practical things as crop yields, water control, and farm planning. This evening he was in the second mood and said, "Very few soil maps can be used for detailed planning of a farm." Ray Roberts was there and he felt that we should get back to the old one-inch-to-the-mile maps and not worry about farm planning. Weeks of Kentucky, and later of TVA, said he felt that the Soil Survey should not aim at accommodating the farmer but only to give fundamental information about soils to those who do work with farmers. Then James Thorp also argued that the soil

classification and soil maps should be simple and deal basically with the internal soil characteristics, leaving the more superficial characteristics that are essentially observed to those actually engaged in farm planning.

Since this conversation took place late in the afternoon I did not answer them. The next morning I did answer them in considerable detail. I reviewed how I happened to get interested in soil survey work in the first place, which was mainly to help farmers and others select soils for use and then to help them discover the most effective management systems. I even reviewed the attacks I had been under from the two extremes: (1) Those who wanted some kind of "pure-science" soil survey that would not be contaminated with the difficult problems of phase definitions and of interpretations, and (2) those who said they wanted only a "practical map to use without all of the fol-de-rol of soil profiles, Podzols, Chernozems, and the like." I explained that those soil survey organizations that had gone along either of these extreme lines had utterly failed. If soil surveys are not based on firm science they cannot be interpreted. If they

are not interpreted for use, the funds dry up. Every administrator, and congressman who supports funds for our work, does so on the assumption that it will aid more effective land use. I asked the boys, "What do you suppose we talk about across the tables in the Bureau of the Budget or in the Congress?"

I pressed them pretty hard and explained that at any time I felt that the Soil Survey didn't have as its aim improvement in farm planning and in guiding forestry, range, and engineering I should be one of the first to go to the Congress and recommend that the appropriations be cancelled and that the money be used for something ^{that} would help get a better fit between the soil pattern and the use and mangement of the soils.

I then asked if there were any questions about our policy. There were none and we went ahead in the normal way with the business at hand.

We then went ahead with discussion and decisions on several soil phases, text^{ual} groups, and other definitions and classes that found their way into the Manual.

In the afternoon of the 18th of March I went to Texas A. and M. My original purpose had been to make an address on the College's "great issues" program sponsored by Sigma Xi on Soil and world food production. Knowledge of this program had reached the National Sigma Xi office and they asked me to be an outside examiner of the College because of their request for a Sigma Xi chapter. This turned out to be a difficult job since I had to get the teaching load, time for research, degrees, equipment, and library facilities of the staff in each science department.

I recall that two of the old department heads didn't much like this idea. The Head of Biology just about refused so I said, "All right I'll just put on this form furnished by Sigma Xi that you refuse to give me the information. I'm not asking for a chapter. You don't have to make your peace with me but with your colleagues here." This prospect didn't delight the old boy and he gave me the material, which showed that his men had almost no time for research.

The old boy in physics showed me their X-ray setup in clay mineralogy. I asked him about a new book in the field and whether

they had it. He didn't know. But a young graduate student said, "I'll go and see." His professor said to me, "You don't mean now do you?" I replied that I couldn't think of a better time. The old boy was much relieved when the student returned with the book all properly classified.

It is curious how such things get around on a campus but I heard both of these stories again 14 years later.

I had a conference with the President and explained a few of the weak places and had his assurance that they would be corrected. I turned in the forms with these explanations. Eventually they got a chapter at Texas A. and M.

As usual I had two or three lectures, one to the PMA folks and a seminar in economics together with discussions about our Soil Survey work and a final lecture on tropical soils.

I came home by air March 24.

When I returned home I found a telegram from the UN offering me something like \$20,000 a year to head an economic and country-planning mission in Afghanistan. Fortunately my guardian angel

gave me enough modesty to turn this down on the basis that I was not an economist. The UN people insisted that they had looked into my record and thought I was eminently qualified but I insisted on turning it down, not entirely from modesty but because of my schedule on the Manual and my plans for Europe.

How lucky I was. In the Congressional Record for March 30, 1950, Senator McCarthy denounces Owen Lattimore as a Communist Agent, who has done great harm to the American foreign policy. He claimed that the State Department had sent him as head of a mission to Afghanistan

In a later session there is included a letter of April 10 from Dr. Evans, of the Library of Congress, saying that Afghanistan had asked the United Nations for a mission on technical assistance and economic development in December, 1949. The personnel for the mission was selected by the UN and paid for by the UN.

Early in 1950, Federal-state-local relations in agriculture by Dr. John D. Black was published by the National Planning Association (46 pp. Washington. February, 1950). This pamphlet gives excellent analyses of the various bills introduced in the Congress and other

other proposals for a ricultural reorganization, including those of TVA, the land-grant colleges, and the Hoover Commission. He makes clear how bad the Hope Bill really would have made things. I was glad then for the many hours I had spent on the various drafts.

In April I concentrated on the Alaska bulletin and the Soil Survey Manual with not too many interruptions.

Some time in 1950, I think about April, FAO distributed to member countries a direct reproduction in large format of Conflicting doctrines about soils, which had been published in 1943 in the "Scientific Monthly". Major Ignatieff told me an interesting story about how this came about. FAO had requests for something to counteract the vicious propaganda on organic gardening and that sort of thing. Richard Bradfield and Ted Crowther agreed to prepare such a bulletin jointly with Bradfield doing the first draft. I never did see it but he postponed the job until the last minute and holed up in a motel and wrote it out while the rest of the family did their sightseeing on a vacation trip. This he sent out to Crowther who took violent exception to it in a long letter to Bradfield.

I can understand this if he made half as many mistakes in that as he did in the awful fertilizer bulletin he wrote some three years earlier. Bradfilled wouldn't answer Crowther, nor would Crowther do anything more. So the plan could not move. Anyway FAO gave up and the editor there said, "This piece of Kellogg's is just what we want and it won't cost us anything."

In May Professor G. W. Robinsin died suddenly. His first wife had died sometime before he went to New Zealand and he remarried near the end of 1949. I was hoping very much to see him and the new wife in Amsterdam.

Early in May the Alaska bulletin went to the printer. I was certainly glad to get that out of the way and so was Dr. Nygard.

Dr. Black and I spent four days together with the TVA people beginning May 19. We were trying to get them to recapture the spirit of the Act and become something more than a bundle of functional groups for combined resource development. Some of the points of emphasis to the Board were:

1. Shouldn't the industrial side get coordinate emphasis with farming?
2. How can TVA be re-dedicated to combined resource use?
3. We need to search for the limiting factors in the relations between natural resources and the cultural pattern.
4. We stressed a broader basis of cooperation with the USDA, Army Engineers, Bureau of Mines, and so on.
5. We emphasized the need for greater effort among low income people, which would require industrial jobs as well as agriculture.
6. We emphasized the opportunity with pilot-research farms to discover entirely new combinations of enterprizes.
7. We thought well of the unit-test demonstration farms but the records needed to be kept more accurately to give quantitative results.
3. We thought that greater effort should be made to project the fertilizer program on a truly national basis.

Following the Board meeting on Saturday, Dr. Black and I had dinner with Mr. George Gant and Mr. Gordon Clapp. The following Sunday Dr. Black and I talked out a presentation to the Board on Monday.

After the Board meeting Monday we had some more talk with the staff and I returned to Washington.

While I had been in Knoxville Dr. Simonson had begun work with the President's Water Policy Committee. Altogether he was able to get a good statement in that report on the Soil Survey.

On May 25 I had the pleasant experience of receiving the Department's Distinguished Service Award with Gold Medal. The citation emphasized my analysis of world food potentials and opportunities.

Early in June Mr. George Gant, General Manager of TVA spent nearly all day with me. He had been authorized by the Board to offer me a position to be in charge of all the natural resources work and regional planning. TVA would be reorganized into three operating units - engineering (construction), power, and natural resources and regional planning. I explained to him that I wanted to be a soil scientist, not a full-time administrator. I explained to him that I had great commitments both nationally and internationally as a soil scientist. He said, "You've made one reputation as a soil scientist. Why not make another as an administrator? Don't answer

me now. Fulfill your commitments in Europe this summer. Take time to think it over and let me know in the autumn. But if you don't come I shall leave."

This was a rough decision. The salary was as high as one could have in a nonpolitical government position. But I thought of all of my commitments and of the men in the Soil Survey who had staid with me during the bad days. I thought of FAO and the international opportunities in soil science to meet the food needs of people. I thought of the big job of attracting vigorous men to TVA against the probability of a reactionary administration in the White House in two years. So I wrote out a careful letter turning the opportunity down and mailed it to Gant just before I left for Europe. (And when I returned he had *gone*.) I corresponded with Dr. Black about the matter before I mailed the letter and he strongly agreed with my decision.

Now Dottie and I were working furiously on the draft of the Manual. Besides my own staff leaders who would get copies to criticize, I wrote to about 30 other soil scientists in this country and in other countries explaining that I should like their criticisms. Those who

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~~Those who~~ agreed to do it were to receive copies. By about the 20th of June it was all in good second or third draft. Dottie was to cut stencils on legal-sized paper and get the copies out for review as soon as possible. As I recall, that was about the middle or end of July.

About this time Robert Kellogg graduated from the University of Maryland with honors. He hoped to be accepted at Harvard (which he was) as a Ph. D. candidate in English. Robert had a job at common labor at the Plant Industry Station for the summer and Mary Alice worked there also as a typist. So the youngsters were prepared to take care of the house and garden while Lucille and I were in Europe. (They did a good job, even got a prize from the city for the garden, and Robert wrote that his sister was a "terrific" housekeeper and cook.

Lucille and I left Washington on June 24. When we got to New York we found that the schedule of our plane to Europe had been changed so that we didn't get in the air for Shannon until just after 10:00 p.m. We arrived in Shannon early in the morning June 25

and were met by one of Tom Walsh's boys, Luke Mannion. We learned that the United States was in war in Korea. About one-half of the passengers on our plane simply waited for the next plane back to New York. But Luke piled our baggage into his car and we started on a field trip to Galway at 8:00 a.m.

The detailed itinerary and observations on this journey to western Europe, given in detail in the European Journal, 1950, and also brief summaries for each country were published by "Commercial Fertilizer".

We had a very good time in Ireland despite the rain but Dr. Tom Walsh didn't give me much time to explore the wanderings of Stephen and Bloom in Dublin; but I did get a look at 30 Eccles Street.

Tom Walsh just about squeezed me dry for suggestions on his soil research program. In addition I had a most interesting conference with Mr. Dillon, Minister of Agriculture. We had a big argument over my strong recommendation for subsidies to get farmers to add much more phosphatic fertilizer. He said to me, "I am sure that when our farmers see the results of Dr. Walsh's research they will buy the

fertilizer; but a crofter and his boy can't move a two-ton boulder; for that they must have a machine." But I said, "Mr. Minister, if the crofter doubles his production on poor stony soil he will still have very little. With adequate fertilizer, especially phosphates you can raise plenty of pig feed in Ireland." And so it went. But I found out later that the old boy was playing Devil's Advocate. He went back ^{into} ~~to~~ the ~~trial~~ that afternoon and his government put forward the very subsidy program I had urged.

Late July 2 we went to London, where I got a bit of food poisoning and then on to Lisbon. We spent most of our time there in interesting field trips. The people were very poor and wages were very low by western European standards to say nothing about

the United States. *I had a visit in the Ministry of Colonies, but they were obviously suspicious of Americans and their talk of the "four freedoms".*

We left Lisbon, by train, about noon July 9, crossed the Spanish frontier in the evening, and the French frontier about noon the next day. Georges Aubert met us in Paris a little before midnight.

Originally we had thought to take a few days holiday in southern France but most of the time I was kept very busy at the American Embassy, and especially at the office of the Economic Cooperation Administration (ECA), the successor of the Mutual Security Administration or Point Four.

The Americans were offering technical assistance and other aid to their European allies, including their African territories. They were anxious for me to straighten out misunderstandings in Brussels where I was to go a week later. Also we discussed a joint study of laterite in Africa to be financed by the Americans. I suggested L. T. Alexander for leader and this was actually carried out in 1951. ECA later gave us some money for laboratory work and some subsequent data from the Congo were included. The final bulletin was printed in 1963.

Carl Ferguson had been a highly satisfactory staff man on soils but Texas A. and M. would not extend his leave so I had to recommend some one else. While I was working on this list in the hotel, Lucille said, "Could Arthur Nelson do this work?" So I put his name down

about third after two men that were quite likely to turn it down.

Thus Art had a nice tour in Europe under somewhat better conditions than he had earlier as a sergeant in General Patch's Army.

I also spent some time with Aubert and others at the Office de la Recherche Scientifique Outre-Mer.

July 14 was Bastille Day. It rained all the morning but in the afternoon we went with Prof. and Mme Aubert to Chantilly where we visited Aubert's father and mother and some of their children. The next day, Saturday, was also a holiday and we visited Barbizon, Fontainebleau, and Mulin. In the evening the Arc de Triomphe and the Place de la Concorde were illuminated for the holiday and there was dancing in the streets. The following day was Sunday and we visited St. Germain and Montmartre.

July 19 we went to Brussels and were met by Jurion. I called on Moran of ECA at our Embassy and explained to him my discussions in Paris. Then I went to the INEAC and talked over the projects with Jurion and his staff. The trouble was with the somewhat stupid man, Englebeen, who insisted on talking English very badly. It was

obvious that he and Moran had been unable to communicate. Moran was a lawyer and the scientific terms meant ~~absolutely~~ absolutely nothing to him so I spent the evening with Moran until nearly midnight going over each project in detail and explaining its relationship to the general program of INEAC and to ECA policy.

The next day we all got together in Jurion's office and everything was settled without much trouble. Here I was able to get provision for research in geomorphology in advance of the soil survey, something I had not been able to do in the United States. (But with the real good results in the Congo as an example, I got this work initiated in our Soil Survey in 1953.)

After lunch Jurion explained the financing and organization of the Soil Survey and other soil research in Belgium. Three universities were involved - Louvain, Gembloux, and Ghent. Under Father Baeyens, there was always trouble but now they had a new man who worked with people better - Professor René Tavernier of Ghent. I would see him at the Congress in Holland. Jurion,

LeBrun, de Leenheer (soil chemist at Ghent) showed me an exhibit of soil maps and soil research. From this exhibit I gathered that things weren't done very well. Apparently the soils were examined and described by a regular grid and soil samples were taken arbitrarily according to the hectares of each kind mapped. Samples of crops were taken from contrasting soils in the same fields under the same management to get productivity ratings. I explained that this wouldn't do at all. Each kind of soil had its own alternative combination of practices for best results, including crop adaptability, timing of operations, and so on. By having contrasting kinds of soils in the same field, one kind or the other would be handled improperly. They had overemphasized geomorphology to the neglect of soil characteristics. I hated to be so critical among friends ^{but} the work wasn't too good. (I learned afterwards that reports of the conference got to Tavernier in Holland before I did.)

Jurion told me that the Belgian Government had finally given approval for him to invite the International Society of Soil Science to hold its fifth Congress in Leopoldville. Several companies had

agreed to help with the financing. Now I was sure that I would not put forward the invitation from the United States. Most of our people in the embassies had told me that they doubted many Europeans would have the dollars to come in 1954. (We all underestimated the effects that the Marshall Plan would have on recovery in western Europe.)

The next day, July 21, was a national holiday in Belgium and the Jurions took us to their farm in the country near Mons for a holiday.

July 22 we left Brussels for Amsterdam and none too soon. King Leopold III had just returned to Brussels and it looked like a riot. That evening we had a preliminary chat with Edelman about plans for the Congress.

The Congress went well and is fully covered in the European Journal, 1950.

July 31 I went with Norman Taylor to the American Consulate for a United States visa. We had a good deal of trouble about Normans's trip to the United States. His government had refused permission on the basis of too few dollars. But I immediately

wrote to them that we would furnish the dollars. In Amsterdam we felt that he would receive permission. Later when we got to Harpenden I was disgusted to find that they still objected. Sandy Muir said, "You've got to learn that you can't push around these sovereign governments." When I got back to the United States I called the New Zealand Embassy and found that the Minister of Science was crossing the United States and would be staying in a hotel in New York. So I wrote him another letter. The lines were courteous but not the material between the lines. He went on to London, visited Norman and got the ruling changed. Then they gave him money for the trip. So he had this money and that that I had arranged for. Like Aubert he had about \$400 left over - which he left with us for his future use.

Beginning August 2 we started the well-planned field trips in Holland. After that I knew how a soil sample must feel going through a laboratory. Our Dutch guides had whistles and stopwatches to keep everybody on time. On a detailed trip of this sort one is enormously impressed with what the Dutch have made out of a little poor soil, abundant water, and a lot of sweat. If all farmers in the world

did as well there would be no world food problem.

August 12 Lucille went ahead to Harpenden for visits in Britain and the next day I started the field trip in Belgium.

Not until I got to Holland was I told about the financial troubles of the Belgian Soil Survey. It seems that IRSIA had decided to abandon it as of July 1. Jurion had encouraged them to go ahead for the next quarter. He told them that I would be there and that I could give them an appraisal, after which they would perhaps be in a better position to decide.

One of the most interesting things that I saw in Belgium this trip was the area of "rounded fields". The soil was naturally a bit ill drained and fields were small. For centuries these were spaded, beginning in the center and working to the outside. This left low ditches around each field that had the shape of a low mound.

August 14 at the conclusion of the field trip Tavernier took me to Brussels and I had a meeting with part of the Board of IRSIA including Prof. van Straelen and the Director, Dr. Henri. We went over the soil survey program in Belgium in detail and I made several

suggestions including one for Prof. Tavernier to spend some time with us in the United States in the autumn. I had been well impressed with him. He was weak in ~~agriculture~~ agriculture and in soil morphology. (It didn't take him long to overcome these weaknesses.) The Board approved the plan and actually increased the budget.

In the afternoon I took the plane to London and joined Lucille in Harpenden chez Muir. I was on the edge of exhaustion and cancelled a proposed trip to Scotland for at least one day of rest.

August 22 I went into London, called at the American Embassy and attended a conference about the proposed ECA projects in Africa at the Ministry of Colonies. The big trouble was that the administrators in the Colonial Office wanted only to get Americans to fill positions. I explained, gently at first and very firmly later, that I would oppose sending any American unless there was a competent opposite number and a junior staff to be trained. They told me they had opposite numbers and I asked for the names. In effect, the administrators suddenly became dignified and informed me that I was dealing with a

sovereign government that could be trusted. Mr. Clay saw my point but kept reminding me of his title - Agricultural Advisor (sic) to His Majesty's Minister of Colonies. While in Britain I had other conferences with our people at the Embassy and insisted on this position. Yet shortly after my return in the autumn the same projects were forwarded to Washington. I was again brought into the discussion and insisted on names of opposite numbers and junior staff. These were furnished in only two or three instances and thank heaven ECA backed me up. No one was sent to the other places.

Since some of the people in Washington were keen to make a soil survey in Fiji, I had recommended instead to the Minister of Colonies that it should be done by New Zealand. At the suggestion of Mr. Clay I went over to the headquarters of the aerial photographic service.

The work was fine except for the paper. I called Mr. Clay and explained that if they could not get ^{dimensionally} ~~dimensionally~~ stable paper, needed in wet-dry areas, they could get it through ECA I felt sure.

(On two more occasions I had to object to this idea of Americans going to Fiji. Norman Taylor's group finally made the soil survey.)

While at Harpenden Dr. Ligon wrote about more trouble caused by one-eyed Fuller. I felt sorry for him but couldn't do much else.

August 28 we went in to London to get our plane for Iceland. We had called in advance and somebody had lied to us because it wasn't due for another day. We got a room in the Scarsdale Hotel near the Kensington Air Station. The next day I found that a chartered flight was going to Iceland but that the bureaucratic British authorities wouldn't let them take passengers. This really made me angry. I called the Embassy and told them to tell these bureaucrats that if we didn't get on that plane they would get no more help from me. It took an hour for the permission to come through and then we had to go to another air station. But we did get out and arrived in Reykjavik about 11:00 p.m. Dr. Bjorn Johannesson met us and took us to the Hotel Borg.

We had fine field trips in Iceland. Perhaps the most dramatic was the plan for growing bananas commercially under glass. They had found a variety- Chinensis, growing in a British botanical museum that responded to the long days. The houses were heated with

natural hot springs so it didn't cost any more for tropical temperature.

With the Icelanders and the people at our Embassy I worked out a scheme for the first soil survey in Iceland. They were to send a student trainee to the United States for about 6 to 8 months. He and Dr. Nygard would go to Iceland in early summer to initiate detailed soil surveys in several typical spots and make a general soil map of the country. (Actually this would have worked out very well except that Dr. Nygard died before his report was done. This was partly procrastination but mainly because the laboratory work was not completed. ECA was generous and brought Bjorn to the United States and we helped him with the report, especially Orvedal. This was published in 1960.

I sketched out a report covering all soil research and land development for the Minister of Agriculture, Mr. H. Jonasson. This was typed later and sent to him and to ECA. The Minister had it put in the farmer's magazine for Iceland.

September 4 we had lunch with the President of Iceland and Mrs. Sveinn Bjornsson at their farm called "Bessastadar". We had a

look around the farm. They arranged for us to have a visit later at the workshop of Einar Jonsson, a famous Icelandic sculptor. The last minute we were also given some special blood serum of sheep by Dr. Sigurdsson to take to the Rockefeller Institute.

After dinner Bjorn took us to the Keflavik airport and we were finally away just before midnight.

We arrived home in Hyattsville September 5 about lunchtime.

When I got to my office I found that the drafts of the Manual had begun to come in. I also had to prepare an official report on the Congress in Amsterdam (included in the European Journal, 1950).

For the first time since the London Congress Bill Pierre agreed to make a foreign trip, this one to Uruguay for the World Bank.

I found that Webb, state soil scientist for SCS in Mississippi had signed an agreement for a joint soil survey and proceeded to violate it at once with Fuller's backing. The same thing happened in a county in North Carolina. Ralph Cummings was a young man then, recently appointed as Head of the Agronomy Department. He was carrying water on both shoulders and thought he could use his

influence with Parker and Salter to "get me straightened out". On very few occasions have I ever talked so mean to a young man as I did to him. It was too bad about Ralph. He matured a bit slowly. He had to leave North Carolina and went first, I believe, to Peru and later to India for Rockefeller where he did a first-class job.

I spent an interesting couple of days on a visit to Lawrenceville, Tennessee. Hub All^Noway was with us then. I was met in Nashville by Moon, Ligon, Odum, and All^Noway and we drove to Lawrence County.

We met all day September 25 with people from the PMA, Extension, TVA, and so on. The soil survey was being used here by a local organization for more effective county and farm planning. After lunch I spoke to the group and encouraged them all I could.

With the help of Ray Heinen and others I fixed up a leaflet made up of a large folded printed sheet called, Sound land classification rests on soil surveys. In the center of a big sheet was a reproduction of a part of a soil map and it was surrounded by six interpretive maps, in color, all derived from the soil map, including one with the SOC capability groupings. This turned out very well and

made the relationship of the basic soil survey much clearer than long texts. When we got this finished and sent it forward for printing I was amused no end to find that the job was sent for printing to the SCS cartographic division. I can't recall now whether it came out at the end of 1950 or early in 1951.

Early in October I started the laborious job of making the final draft of the Soil Survey Manual, most of it on my dining room table. After the visit in Europe I realized that the soil physicists had completely misled us in their definitions of drainage of the soil. They wrote something like this: "In a well-drained soil the excess water not held in the capillaries passes rapidly through the soil whereas little water moves through a poorly-drained soil". This would make the soils of the desert poorly drained and the hilly bog soils of Ireland and Scotland well drained. I realized that we needed to define a concept of "soil drainage" in terms of the degree of water-logging. I could find relatively few data for giving quantitative definitions to the seven classes that were needed in at least some soils.

This was put into the Manual de novo without field testing. A few soil scientists have expressed some dissatisfaction with the scheme but no one has suggested a substitute. This certainly was a vast improvement.

Instead of having a glossary I decided to use special type in the index for the page numbers where definitions were given. I recall going through 45 of the mimeographed copies of the draft. Some drafts had many suggestions; others only a few; but all were helpful. With any slight change in a definition the manuscript had to be searched for every place the word was used to be sure there were no errors. What a job! I spent every moment that I could on this manuscript and on dictating the European Journal, 1950.

I attended the Soil Science Society meetings in Cincinnati between October 31 and November 3. I presented a report to the Society on the Congress based on my longer report to the Department of State.

Tavernier, Norman Taylor, and Focan were all here in October. In fact we had both Taylor and Tavernier as house guests for a while.

This complicated work on the Manual but we had a good time. Every day was Christmas. Again that year we had many foreign visitors including Elliot of New Zealand, Felix Richard, J. B. Watson, and several others. Taylor and Tavernier were on field trips much of the time and left a few days before Christmas. The only speech I can recall of that period was one on Garden Soils to the National Capitol Garden Club of Washington.

I concentrated very much on the Manual. I wanted this well indexed so I started to prepare it from the manuscript by dictating words and phrases as I came to them on cards that were numbered consecutively. It was an enormous stack. These were put together alphabetically so I could make a draft of the index. I don't know about other people but in this process I made some errors by using slightly different phrases for the same concept. Incorrect cards were adjusted and they were put back again in the order of their numbers, ready for the page proof. I was finally able to turn the manuscript over to the editors for printing about the middle of December.

Since this routine of working nights and weekends on my dining room table was well established, and tolerated by Lucille (Robert was at Harvard and Mary Alice was living at the Tri Delt House in College Park), I pulled out the partly done manuscript on garden soils and went after it. The biggest job on this was working out the soil requirements for the principal plants grown in gardens in the area from about Raleigh, North Carolina to Lincoln, Nebraska, to Madison, Wisconsin, and over to Portland, Maine.

1950 closed with the half-completed book on garden soils on my dining room table.

1951. In January I got ^a caught up a bit on my other work at the office that had been neglected while completing the Manual and worked steadily at home on the garden book. Mary Alice helped me by typing the manuscript.

As the year began there was a good deal of speculation about who Bennett's successor would be. Bennett himself was pushing very hard for Edward Graham an emotional ecologist completely lacking in administrative acumen. Farm magazines were predicting a soil scientist. Some predicted Salter for this job and some predicted that he would be research administrator and that Kellogg would succeed Bennett. This latter would really have put the cat with the pigeons since Bennett, Norton, and some of the others in the SOS Washington office had spent a large part of their time in explaining my responsibility for everything that went wrong. (This view was not shared by the younger men.) It gradually became clear to me that Salter would ^{be} probably be chosen providing Secretary Brannon could face the political pressure of the men in the National Association of Soil Conservation Districts whom Bennett had made. This didn't look too good because

he had already extended Bennett a year beyond mandatory retirement at 70. The decision had been taken to move the Food and Agriculture Organization of the United Nations (FAO) to Rome and they moved early in 1951. Several of us had been worried about this from the very beginning. At the Quebec Conference I had urged for a site to be chosen in northwestern Europe instead of Washington. It seemed clear that if the UN headquarters were in the United States, Italy would be bound to exert pressure for FAO to take over the remains of the old International Agricultural Institute. This would be a bad location because FAO would have available nearby almost no first-class professional people as compared to Washington or to some European city in the northwest, such as Copenhagen, Amsterdam, Brussels, or London. If such a site had been chosen in the first place no move would have been necessary.

^{14th}
~~In~~ February, I went to Des Moines, Iowa to take part in the National Farm Institute sponsored by the "Des Moines Register". As I recall I spoke on the role of scientists in helping the newly-developed countries achieve agricultural abundance. I had an evening session with Jim Row of "Successful Farming" and agreed with him on a

popular paper dealing with the problem of the poor farms. After the Des Moines meeting I went to Ames with Pierre and some of his staff and had conferences about the cooperative work.

This March we decided not to have the usual Work-Planning Conference. We were expecting galley proof on the Manual and didn't expect to make many changes in it anyway.

We got the laterite study set up for Africa with Dr. Alexander heading the mission.

On March 3 we sent the manuscript for the garden book to Macmillan for their review and comment. By now it had a name - Our garden soils. They spent quite a lot of time checking with one or two readers and calculating costs. We had a bit of correspondence for two or three months about tables and so on.

The day after the manuscript left, Dr. E. M. (Ted) Crowther of Rothamsted came to pay us a visit for nearly three months. He staid at our house during the first of his visit and at the end in the middle of May. We enjoyed him enormously partly because of his excellent taste and his excellent sense of humor.

During the winter of 1950-1951, as I recall, during a chat ✓
with Dr. Byron T. Shaw, Assistant Administrator of Research, he
complained almost bitterly about the lack of recognition of the
Agricultural Research Administration. He said, "People know about
the Bureau of Plant Industry, the Bureau of Animal Industry, and
the other research bureaus, but who ever heard of the ARA?" At
the time I thought he was in a moment of pique about something.
Later it was clear that he was planning then to do away with the
bureaus, primarily to make people deal with the office of the
Research Administrator.

February 15, 1951, Secretary Brannon signed memorandum 1278 on
Coordination of the Department's Agricultural Resources Conservation
Services. This defined the relationships among the Production and
Marketing Administration, the Soil Conservation Service, and the
Forest Service. It had been sweat over a good deal. It also
provided for consolidated state and county offices. The Soil
Conservation Service was given technical responsibility "for all
technical phases of the permanent type of soil conservation work,

except forestry" undertaken by either SCS or PMA. Other provisions were spelled out more carefully than heretofore.

Later I learned that at the same time Shaw proposed to include a scheme that would have eliminated the research bureaus. But it was refused. This idea, however, awaited a new and less able Secretary of Agriculture and led to one of the worst mistakes and resultant eras of confusion in the history of United States agricultural research.

L. R. Schoenmann had asked me to give the principal lecture at the meeting of the Michigan Academy of Science, Arts, and Letters being held at Michigan State. I got there the day of the lecture, March 23. I had luncheon with a group of economists and in the afternoon gave my lecture on Scientists creating opportunities for world abundance. (This was later reworked a bit and published in the "Journal of Politics" of that year as Opportunities for world abundance.)

On April 4 Robert was married to Joan Montgomery. It was a simple ceremony with only Joan's parents, her sister and brother-in-law, and Lucille present. We both felt that Robert had made an excellent choice. Joan was working as a chemist at Sylvania and he continued his graduate study at Harvard.

The galley proof for the Manual started coming back in April. While I was working on that Dr. Salter suggested that the authorship should be changed from my name to "Soil Survey Staff". He said, "This change might lead to better acceptance of it within the Department even though it might lower its authority outside the

Department". At first this irritated me a good deal. Yet I was pretty sure that he was going to SCS and he thought that they would accept it better. To make matters worse Parker spoke to me about it a few days later and said, "It is a pity that SCS, and especially Bennett, has so little confidence in your judgment." Now, of course, Bennett had no lack of confidence in my judgment. Instead he was revengeful against me because I had opposed the transfer of soil research to SCS over the years, especially between 1936 and 1942. Unhappily, one can't do much about these things but had I agreed neither Salter nor Parker would have had their opportunities. Had I compromised with Bennett, he would not have treated me any better and my friends would have deserted me.

I talked about this suggestion with several people in confidence and none of them agreed with it. But since the question had been raised I decided to go ahead with the change. As it turned out practically everyone knew who had designed it and written it and probably the formal authorship never mattered.

In April I prepared a first draft of a proposed unified soil

File

May 1, 1951.

Charles E. Kellogg, Chief, Division of Soil Survey

Soil Survey Manual

Now that I am reading the galley proof of the revised Soil Survey Manual, it is a good time to summarize its development.

In 1947 I began definitely to gather the material together for a new edition. Previous to that, for 10 years, I had established committees of the staff to prepare new or revised statements of various aspects of research involved in soil surveys, including the basic morphology, classification, mapping, publication, and interpretation. None of these individual statements were acceptable as first written, and I had the staff rework most of them several times. Other highly qualified soil scientists outside of the Division were asked to review the statements and comment on them, including State soil survey leaders and soil survey leaders in other countries.

After 1942, SCS was invited to send representatives to our Soil Survey Work-Planning Conferences. Mr. Hockensmith attended and took part in the discussions. Later, one representative from each Land-Grant College region took part.

In 1947 I made a thorough review of the materials of the Conferences as well as the world literature on the subject in order to identify any important gaps for which staff committees might be established, and to guide my own reading and research. I also made a tentative outline of the new edition to which several members of the Beltsville staff reacted. After some revision, it was sent to all the senior staff and some State scientists for their reaction and was decided upon definitely April 20, 1948. Most of the previous committee reports did not fit into this or any other orderly outline; therefore, some individuals who had been identified in the committee work with particular subjects were asked to prepare revised statements that would be more useful to me in preparing the Manual. These statements covered somewhat less than half of the outline. A few of them were directly usable, but not many.

In the autumn of 1949, about November, I had all other work cleared away as well as possible in order to concentrate on the writing. Since the demands on my office are exceedingly great, most of the writing had to be done at home, either in rough outline and redictated in the office, or dictated into a recording machine. My home schedule was rearranged to permit me to work on this all the Saturdays, Sundays, and holidays that I was in town, between

Charles E. Kellogg, Chief, Division of Soil Survey

Soil Survey Manual

Now that I am working the latest draft of the revised Soil Survey Manual, it is a good time to summarize the work.

In 1937 I began working to revise the manual. It was a new edition. I worked on it for 12 years. I was a member of the staff in various capacities. I was involved in various aspects of research involved in soil surveys, including soil morphology, classification, mapping, publication, and interpretation. None of these involved the same work as the first edition, and I had the staff working on the first edition. When highly qualified soil survey leaders in various states were asked to review the statements and work on the including State soil survey leaders and soil survey leaders in other countries.

After 1942, SCS was invited to send representatives to our Soil Survey Work-Planning Conferences. Mr. Hockensmith attended and took part in the discussions. Later, one representative from each State-Survey College began to work.

In 1947 I made a thorough review of the materials of the Conferences as well as the work of the staff. I was a member of the staff in various capacities. I was involved in various aspects of research involved in soil surveys, including soil morphology, classification, mapping, publication, and interpretation. None of these involved the same work as the first edition, and I had the staff working on the first edition. When highly qualified soil survey leaders in various states were asked to review the statements and work on the including State soil survey leaders and soil survey leaders in other countries.

In the summer of 1947, when I was at home, I had to work on the manual. I was a member of the staff in various capacities. I was involved in various aspects of research involved in soil surveys, including soil morphology, classification, mapping, publication, and interpretation. None of these involved the same work as the first edition, and I had the staff working on the first edition. When highly qualified soil survey leaders in various states were asked to review the statements and work on the including State soil survey leaders and soil survey leaders in other countries.

breakfast and dinner. Revisions of drafts were done in the evenings. This schedule was maintained until the beginning of June 1950. As first drafts were completed and revised, from one to three individuals were asked to review them and make comments. Thus in June 1950, I had all parts of the revised Manual in either a second, third, or fourth draft.

Since I had to be away for about 11 or 12 weeks during the summer, these drafts were roughly edited by the editorial office and mimeographed by my office. In August, about 45 copies of the mimeographed draft were circulated to prominent experts in soil survey work. Perhaps roughly half of them went to the senior staff of the Division and the others to soil scientists in State soil survey organizations and in foreign soil survey organizations. One went to the other soils division of the Bureau and one to the Geological Survey. About 40 of these were returned. Most of them had at least a few comments, and about 15 or 20 had been gone over very thoroughly.

In the meantime, I had an opportunity to think more about some of the items and especially to discuss them with informed people in Western Europe as well as with prominent soil scientists attending the Amsterdam conference and excursions.

Shortly after my return to the United States in September of 1950, I began a detailed revision of the mimeographed draft, mostly at home with the same work schedule that I had followed in the spring. By the first week in November, most of the suggestions had been received and several chapters rather drastically revised and reorganized. At this stage, a few of the chapters went to especially informed people in the country for a second look.

The whole was completed just before Christmas and turned over to the editorial office of the Bureau for final editing and marking. This took somewhat more time than we had anticipated, partly because of the large amount of retyping that was necessary, all of which I had to check again very carefully. As I recall now, it went to the printer near the end of February or early part of March. It is difficult to say precisely how long the manuscript was that went to the printer. Since a great deal of it will be put in fine type, it will probably work out to a book of around 500 pages.

Since the Manual is a revision of the one written by me and published by the Department in 1937, and since the final manuscript was written entirely by me, it of course carried my name as the author, with full credit to all staff members and other scientists outside of the Division who had made substantial contributions.

the all parts of the revised Manual in either a second, third or

There are no other copies of the manuscript in the library of the University of Toronto.

[illegible]

The above was completed and signed by the
 undersigned on the 10th day of June 1964.
 JAMES H. HARRIS, JR., Mayor
 I, the undersigned, certify that the above
 is a true and correct copy of the original
 as the same appears in the files of the
 City of New York.
 JAMES H. HARRIS, JR., Mayor
 I, the undersigned, certify that the above
 is a true and correct copy of the original
 as the same appears in the files of the
 City of New York.
 JAMES H. HARRIS, JR., Mayor

The following list of authors was sent to the President of the American
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 of authors who have written the book in whole or in part.

File-3-5/1/51

On Friday afternoon, April 6, while discussing another matter on the telephone, Dr. Salter, Chief of the Bureau, suggested that it would be better for the Soil Survey Manual to appear anonymously from the Soil Survey staff rather than carry my name. He said that perhaps this would lead to better acceptance of it within the Department, even though it might lower its authoritativeness outside the Department. Obviously, this came as quite a shock.

During the early part of the next week, I discussed the matter confidentially with several people intimately familiar with the work. They did not have a similar view, nor did I. The matter was discussed briefly again with Dr. Salter April 13, and he insisted on the point of view previously expressed.

From the conversations, I got the definite impression, perhaps incorrectly, that the idea had been suggested to Dr. Salter by someone else. At least, in a conversation with Dr. Parker a few days later on another matter, he said that "it was a pity SCS, especially Bennett, had so little confidence in my (Kellogg's) judgment". Now, of course, Bennett has no "lack of confidence in my judgment;" he is revengeful against me wholly because I opposed the transfer of soil research to the SCS in the years 1936 to 1942.

After thinking the matter over two or three days, I decided that since the question had been raised, it would probably be better to remove my name as author and substitute "Soil Survey Staff." I simply could not allow a situation to arise in which the welfare of the Soil Survey might be jeopardized by implications, no matter how unfair, of vanity and stubbornness on my part.

Thus, as I have the galley proof before me now, it is going back to the printer as if written by the Soil Survey staff, together with the elimination of all names in the introduction and with general statements instead indicating that assistance was received from the heads of State soil survey organizations and from those in foreign countries. Of course, revision is also necessary of some individual sentences in which I had referred to more or less personal ideas and experiences. Since I had been careful to give credit for new ideas to other members of the staff and other scientists, the manuscript as it now appears does not indicate that I either wrote it or in any way contributed to it! Although the fact that I have written it will be obvious to persons familiar with my other publications.

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survey in the Department of Agriculture partly to be ready for the conferences that seemed inevitable and that might come on short notice. This did not necessarily involve many transfers of people or funds among agencies.

Thorp, who was principal soil correlator for the Great Plains states, asked for leave of absence to accept a professorship at Earlham College. He wanted the best of two possible worlds - a professorship at Earlham and full civil service retirement. I had to tell him that I couldn't possibly leave that job open so he staid a while longer.

Early in May I learned that R. M. Salter was definitely to become Chief of the Soil Conservation Service on July 1, next. I had suspected this for several months, beginning especially with the confidential conversation with Secretary Brannon on Fifth Avenue, New York, during Saturday morning, August 20, 1949. The solution he sought could not easily have been effected in any other way than by the appointment of Salter as soon as it became possible. Heinen and I

did our best to see to it that Salter's speeches and the other announcements of the Bureau would strengthen the conviction that this ^{sh}ould be done. So it didn't come as a surprise.

John McAuley told me that Salter was anxious to see my own grade changed to that ⁹equivalent of the Assistant Chiefs of the Bureau before he left, on the basis of scientific leadership. I prepared drafts for this purpose over the weekend and gave them to McAuley Monday, May 21. Over the weekend I had also put into second draft a statement of why the Department should have one unified soil survey organization and why it should be within the ARA. Copies of this draft were sent to Salter, Barnes, and Parker on Tuesday, May 22. That same day I had a long conference with Dr. Salter and he explained in more detail the arrangements for him to be Chief of SCS beginning July 1, next, and that he expected to take Mr. Heinen and Dr. Omar Kelley with him. During the previous months Dr. Salter and I had had many conference^s about what should be done in SCS, and I found that he had already taken up some of these with the Secretary

and had agreement. One of them was that this propaganda deluge should stop. Another was that a committee of distinguished agriculturists from various walks of life should be established as an advisory committee to the Chief of the SCS and to the Secretary. This all looked very good for the future of soils work, and I strongly urged Dr. Salter, probably against my own immediate interests, not to make any sudden moves in the SCS on the three critical matters that concerned me most: The soil survey program, TVA relations, and the soil research program. Dr. Salter suspected that there were few men on the SCS staff in Washington with competence and integrity. At the same time the SCS men lacked courage, and we both felt that they would not do anything to make trouble. There were some good men in the field, and he thought he would gain their confidence by moving slowly rather than rapidly.

One very disturbing feature of my conference with Dr. Salter on Tuesday, May 22, was his statement that no decision might be made about the Chief of the Bureau of Plant Industry, Soils, and Agricultural

Engineering for several months. It seemed that Byron Shaw, Deputy Research Administrator, was very uncertain about his own position. If Dr. ^{Carson} ~~Carson~~, the Administrator, should retire in 6 or 7 months, it seemed doubtful that the Secretary would appoint Shaw. Shaw wanted to keep this Bureau position open so that if he didn't like the Administrator that was appointed, he could take the job as Chief of the BPISAE.

The more I thought about that last point, the less I liked it. The Bureau already had some very difficult prima donnas, especially Turner and Parker, and several months with a weak, uncertain-acting chief could be enough to get the Bureau in a mess. Obviously Dr. Cullinan was not qualified because of his lack of general education and inability to reach a decision. Others in the Assistant Chief grade were all specialists with the exception of Dr. Moseman, who was very young. The next day I had a private conversation about this matter with Dr. Moseman, and he shared my great concern. Here we were trying to establish some Bureau ^uprojects across division lines. This requires a strong Chief, especially to deal with the prima

donnas. Also, if Dr. Salter made the kind of reorganization in SCS that would be best for effective work, the ARA, and above all this Bureau would need to reorganize its research work in ways to give the maximum help to the conservation program - I hoped a cooperative program among SCS, PMA, and Extension. This could not be done except by a strong hand. I therefore came to the firm conclusion that Dr. Roseman should be appointed Chief of the Bureau despite his youth immediately after Salter left.

I knew that Dr. Cardon was leaving for Rome on Friday, May 25, so I called him at his home Wednesday evening, May 23, and told him that I was disturbed about this situation. I was amazed to find that he didn't know that a definite date had been set for Dr. Salter's transfer to SCS. I couldn't imagine why Shaw had not informed him. He also knew nothing of this scheme of Shaw's to leave the chiefship vacant. Of course, I can see why Shaw didn't inform him of that, and was glad he didn't. I then made the definite recommendation about Moseman. Of course, Dr. Cardon didn't commit himself and shouldn't have, but he did say that it was a very good suggestion. He agreed

on the others not being qualified. He said he was sorry that
Moseman didn't have two or three more years behind him and I said
I was too, but we were dealing with a real situation and not with
the ideal.

Thursday, May 24, I had further brief talks on this matter with McAuley and Moseman and pointed out to McAuley that Cardon was leaving Friday, and he agreed to get Cardon's approval of my own papers before Cardon got off to Rome, since that might hold them up for several weeks.

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Parker said: "Well, you are talking about fertilizer technology; let's confine our attention to the soil management phase." I said: "All right. If you and Bob Parks have some ideas, get them together and send Bob over to talk with me, and I will write a letter to Mr. Moon." I couldn't resist pointing out that that would save time, since Mr. Moon would write to me about it anyway. I never did learn exactly what Parker's game was in all of this. He had always refused any help to TVA before.

Journal Memoranda

Because of several developments that may have far-reaching consequences, I am jotting down the principal points while they are fresh in mind.

About Wednesday, May 16, I learned from John McAuley that R. M. Salter was definitely to become Chief of the Soil Conservation Service. I had suspected this for several months, beginning especially with a confidential, private conversation with Secretary Brannan on Fifth Avenue, New York, during Saturday morning, August 20, 1949. The previous day H. H. Bennett had given an address before the UN conference on natural resources, about one-half of which was devoted to criticism of my work, although he did not mention my name. The Secretary had understood this and he commented to me bitterly and with considerable invective of his complete lack of confidence in Bennett and his organization. The solution he sought could not easily be effected in any other way than by the appointment of Salter as soon as it became possible. I did my best to see to it that Salter's speeches and the other announcements of the Bureau would strengthen the conviction that this should be done. So it didn't come as a surprise.

McAuley told me also that Salter was very anxious to see my own grade changed to that equivalent of the Assistant Chiefs of the Bureau before he left, on the basis of scientific leadership. I prepared drafts for this purpose over the weekend and gave them to McAuley Monday, May 21. Over the weekend I had also put into second draft a statement of why the Department should have one unified soil survey organization and why it should be within the ARA. Copies of this draft were sent to Salter, Barnes, and Parker on Tuesday, May 22. That same day I had a long conference with Dr. Salter and he explained in more detail the arrangements for him to be Chief of SCS beginning July 1, next, and that he expected to take Mr. Heinen and Mr. Kelley with him. During the previous months Dr. Salter and I had had many conferences about what should be done in SCS, and I found that he had already taken up some of these with the Secretary and had agreement. One of them was that this propaganda deluge should stop. Another was that a committee of distinguished agriculturists from various walks of life should be established as an advisory committee to the Chief of the SCS and to the Secretary. This all looked very good for the future of soils work, and I strongly urged Dr. Salter, probably much against my own immediate interests, not to make any sudden moves in the SCS on the three critical matters that concern me most: the survey program, TVA relations, and the research program. I pointed out that there were no men on the SCS staff

in Washington with either competence or integrity. Dr. Salter agreed. At the same time they also lack courage, and we both felt that they would not do anything to make trouble. There are some good men in the field, and he would gain their confidence by moving slowly rather than rapidly.

One very disturbing feature of my conference with Dr. Salter on Tuesday, May 22, was his statement that no decision might be made about the Chief of the Bureau of Plant Industry, Soils, and Agricultural Engineering for several months. It seems that Byron Shaw, Deputy Research Administrator, is very uncertain about his own position. If Dr. Cardon, the Administrator, should retire in 6 or 7 months, it seems doubtful that the Secretary would appoint Shaw. Shaw should rather keep this position open so that if he didn't like the Administrator that was appointed, he could take the job as Chief of the BPISAE.

The more I thought about that last point, the less I liked it. The Bureau already has some very difficult prima donnas, especially Turner and Parker, and several months with a weak, uncertain acting chief could be enough to get the Bureau in a mess. Obviously Dr. Cullinan is not qualified because of his lack of general education and inability to reach a decision. Others in the Assistant Chief grade are all specialists with the exception of Dr. Moseman, who is very young. The next day I had a private conversation about this matter with Dr. Moseman, and he shares my great concern. Here we are, trying to establish some Bureau projects across division lines. This requires a strong Chief, especially to deal with the prima donnas. Also, if Dr. Salter makes the kind of reorganization in SCS that is required for effective work, the ARA, and above all this Bureau, shall need to reorganize its research work in ways to give the maximum help to the conservation program -- I hope a cooperative program among SCS, PMA, and Extension. This won't be done except with a strong hand. I therefore came to the firm conclusion that Dr. Moseman should be appointed Chief of the Bureau despite his youth immediately after Salter leaves.

I knew that Dr. Cardon was leaving for Rome on Friday, May 25, so I called him at his home Wednesday evening, May 23, and told him that I was disturbed about this situation. I was amazed to find that he didn't know that a definite date had been set for Dr. Salter's transfer to SCS. I couldn't imagine why Shaw didn't inform him. He also knew nothing of this scheme of

Shaw's to leave this chiefship vacant. Of course, I can see why Shaw didn't inform him of that, and I am very glad I did. I then made the definite recommendation about Moseman. Of course, Dr. Cardon didn't commit himself and shouldn't have, but he did say that it was a very good suggestion. He agreed on the others not being qualified. He said he was sorry that Moseman didn't have two or three more years behind him and I said I was too, but we were dealing with a real situation and not with the ideal.

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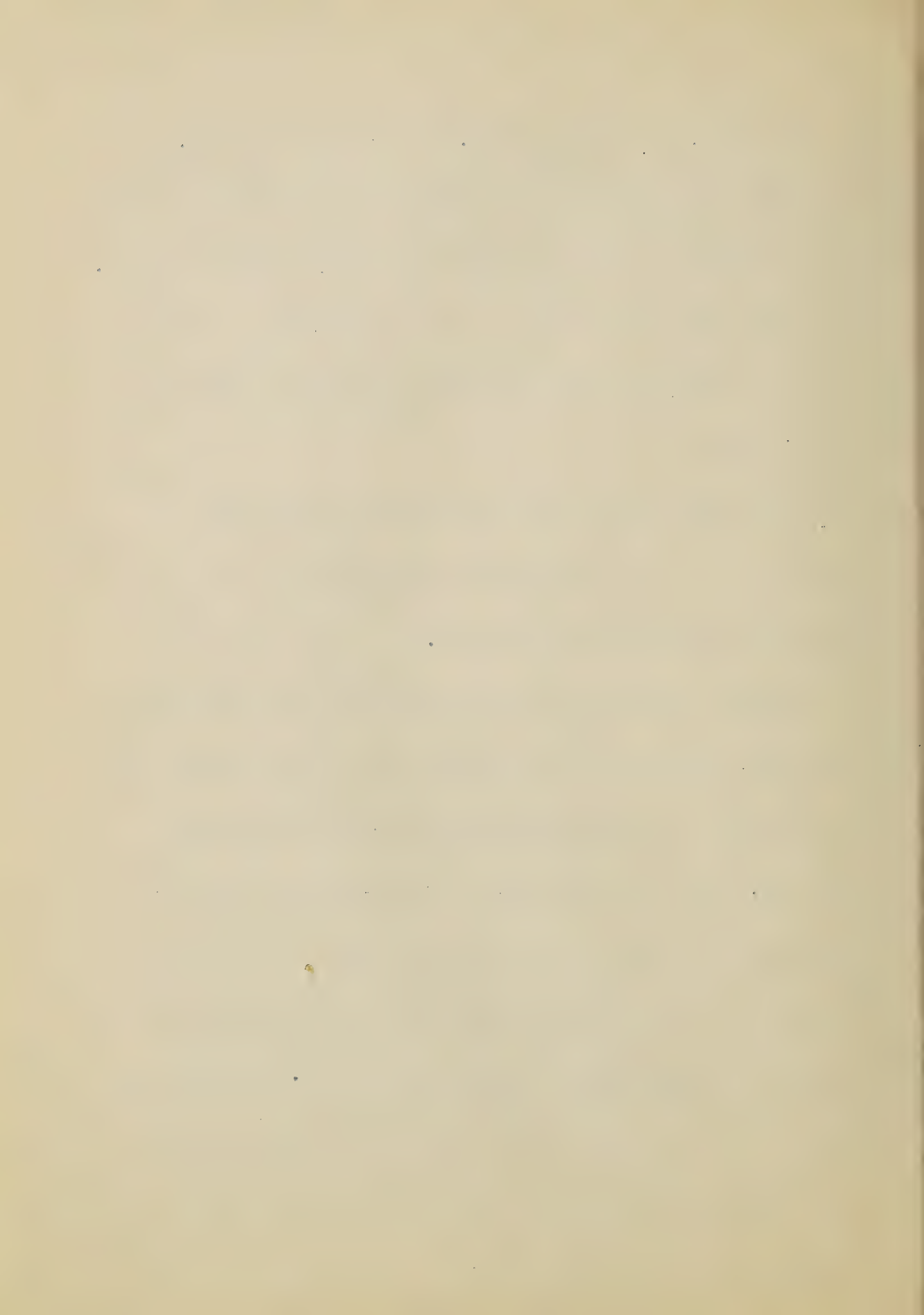
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In May Dr. M. A. McCall retired. I hated to see him leave. He had always been a tower of strength in the Department and to me personally.

We still had a lot of work to do on the galley proof of the Manual. I had sent copies to only two people, Dr. Marlin G. Cline, and to Dr. Guy D. Smith. They both had some suggestions that I could only partly accept.

Also in May I worked with Dr. Black on his material about agricultural land and fertilizer that he was preparing for the President's Materials Policy Commission.

Crowther finished his tour and staid with us a while before he went back. He was a good mimic and gave Lucille and me some wonderful laughs about his visits and especially his attendance at Rotary meetings. He had been at several of our top places so I asked him to tell me which one was the best on strictly intellectual grounds. He replied, "That's an easy question; by all odds I should say the group in fertilizer research in TVA at Muscle Shoals," I remember also that he was very much impressed with Max J. Edwards who had worked with me in North Dakota and was then with Dr. Ligon in the Knoxville office of the Soil Survey.



"Edwards is a man", he said, "who knows what he knows and what he doesn't know. One can trust him." Ted was so right.

I believe that it was near the middle of May that the galley proof went back on the Manual. As I recall the page proof came in June and nobody worked on that except Dottie and me. We had the cards all ready and it wasn't too big a job to fix up the index with bold-faced type for the definitions.

In June we had a conference with TVA including Moon, Bass, Rogers, and Winters along with Kellogg, Parker, Dean, Alloway, and Moseman for the Bureau. Almost immediately Parker suggested that all of TVA's responsibility for designing experiments and for interpreting the results be turned over to the Bureau. With patience the men from TVA explained why this wouldn't be practicable and the discussion turned on the need for a TVA man to work actively in the program too. It was agreed to have a short memorandum setting forth the mutual interests and the desirability of working with the regional committees.

That evening I talked more with Moon about this. He told me that

before coming he had discussed the matter at length with the Board of TVA. The chairman indicated great suspicion. He pointed out that Parker had been opposed to their program and given aid and comfort to those criticizing TVA all these years. He said, now that our difficulties are about over and we have a good going program cooperative with the states, the Atomic Energy Commission, individual fertilizer companies, and the National Fertilizer Association - now Parker says he wants to cooperate, now when we don't need him. Is there a catch? And this is the question that Mr. Moon proposed to me. I had to tell him that I didn't know. I assured him that I wouldn't recommend anything to TVA that I wasn't satisfied to be on the up-and-up. And to be satisfied, I should have to have it in writing and approved in the Office of the Chief of the Bureau. I suggested that he tell the Chairman that the cooperation we were proposing now was so much in the interest of TVA, of the Department, and of the American farmers, that none of us, including the Chairman, should permit one personality to block it. I was sure that there would be no difficulty with any of the men actually doing the work in TVA, in the states, or in the

June - July

Bureau. During the process of reaching an agreement, the Chief of the Bureau would be kept fully informed and the arrangements fully documented.

Mr. Moon suggested that he should like to have me come to Knoxville in the rôle of consultant to TVA in order to discuss the matter with the Board before final agreement was reached. I told him I thought I could do that in July.

Early in June I again reworked my confidential memorandum on a unified soil survey in the Department and gave a copy to Dr. Salter.

Also I had additional work with Dr. Black on the land and fertilizer section of his report on the President's Materials Policy Commission.

Mary Alice graduated with honors from the University of Maryland in the middle of June. Directly after that she went to work with the International Monetary Fund.

July 1 came and Salter had no word to move to the **SCS**.

In July I was formally notified by Macmillan that Our garden soils would be printed by them and they sent me a check for one-half of the initial payment.

Near the end of July I went to Knoxville to discuss the agricultural program with Moon and his staff and the general administrative problems of agriculture with the Board of TVA. The first evening I spent with Moon and urged him to avoid his continual self depreciation; and that when problems came up to make specific recommendations to the Board after study.

On August 4 I returned the manuscript for Our garden soils with the adjustments suggested by the editors at Macmillans.

I had a telephone call about August 12 from Joe Moon indicating that some of the state people, especially Pierre and Cummings, were rather objecting to the proposed participation of the Bureau in the planning of the fertilizer testing with TVA. They were afraid of the combined federal authority and told him they didn't need the benefit of Parker's group on experimental design. On August 13 I wrote to Joe explaining why I felt that he shouldn't pay too much attention

to their criticisms. On the same date I wrote a long critique to Neil Bass on the proposals that had been forwarded by Secretary Brannon to the Board. A conference was scheduled to discuss it during the time I was planning to be in northern Canada. In this long critique I pointed out what I thought TVA could accept and what I thought they ^houldn't. It was not a very good letter from the Secretary considering the background of suspicion that TVA was bound to have after all that had gone before.

In 1951 the Society meetings were scheduled for Pennsylvania State University in August. Since rumors had already gotten abroad that Salter was to become Chief of the SCS I didn't want to go to them. I knew that people would be asking me about these rumors. I wouldn't want to tell them the truth and I knew that I couldn't lie convincingly. For some time the Canadian soil scientists at Saskatoon and Edmonton had been after me to make a visit in northern parts of Saskatchewan and Alberta to advise them about agricultural development. So I accepted.

(The notes and black-and-white photographs for this visit are included in the Canadian Journal, 1951.)

Dr. John Mitchell and Mr. H. C. Moss met me in Regina, Saskatchewan August 20. The next day we took a leisurely field trip to Saskatoon. I had two days there at the University.

We started for the north the morning of August 24. Just as we were to start I said, "Have you got all the equipment - spades, pH testers, thermometers?" Dr. Mitchell said, "Why do you want a thermometer?"

I replied, "Well we're going to look at soils. Temperature is a soil characteristic and I didn't bring one with me." So he sent one of the boys back after a thermometer.

I was just playing a hunch. John had written me about some of their problems and wondered why the soils of the north responded more to sulphur than those in the south.

We had a wonderful trip through Melfort and on north despite the rain and mud. This was my first real look at the Gray-Wooded soils. I had never been with more delightful companions. When we got up into

the northern problem area we got out the thermometer. The soil temperature was below 42° at around 18 or 20 inches. Although there was gypsum underneath the roots of the crop plants couldn't reach it so they responded to sulphur.

August 30 we drove to Edmonton, Alberta. Moss left the next day for Saskatoon and we picked up Bentley, Bowser, and Newton and went up to the Peace River Country. The soil survey was nearly completed under a party leader named Odynsky, a very active man. As soon as we got started it was plain to see all sorts of tundra mounds and other arctic features inherited from not many thousand years ago before invasion by the present grass on the Chernozems and the forest, dominated by aspen, on the Gray-Wooded soils. I called attention to a tundra mound. Odynsky said, "No. We've had a geologist up here. That is an esker." I didn't argue with him but I kept many notes and made many cross sections from the road cuttings.

That evening of September 3 we had a little seminar in the hotel and I brought out my cross sections and showed Odynsky that during the tundra period the outlet of the Peace was blocked and that the

water must have flowed east and a bit south toward Edmonton. I said, "If that is correct there should be a sandy delta over there." One of the boys from Edmonton spoke up and said, "There is a sandy plain and we can go back that way."

I went over the profiles and cross sections of the contacts between the underlying heavy, gray clay and the upper brown silt loam with them and showed them how the underlying clay had been forced up irregularly by the frost into the surface layers deposited along the old Peace. When the permafrost went out, the Peace returned to its old course to the north and cut such a deep channel that the early terraces were not easily obvious. Poor Odynsky was a bit unhappy over having missed these features but so would many other people had they never seen the active tundra.

September 4 we drove on down to Edmonton. After all the rough roads my back was nearly killing me. The next day we went on south of Edmonton to Red Deer and then over to Hardesty. We left our Edmonton friends September 7 and drove to Saskatoon. The next morning I was up at 4:30 for the 5:30 train but it went out three and

one-half hours late so I missed my connection in Winnipeg. Because of some kind of convention the hotels were full. Several ^{more} ~~less~~ timid people were in the same boat. I insisted that it was up to the railway to spot a pullman car for us in the yard and they did. The other people were so glad that they laughed and talked until midnight while I read the life and works of Sarah Binks, the sweet songstress of Saskatchewan. Still I had a good sleep. The next morning I couldn't get a day ~~hotel~~ ^{le} room but managed to find a writing space. That evening my train came and I went on home.

The Soil Survey Manual was out! And when I first opened the book I saw a photograph upside down. But it was good to be through with this for a while. It was to become the standard reference in most countries.

I found that Salter still hadn't gone to the SCS, but he expected to.

In the current issue of the "Fertilizer Review" the new editor had written, "If anyone had predicted ten years ago that American farmers would be using annually two and one-half times as much

fertilizer today as they ~~consumed~~ then, their sanity would have been questioned." Mr. Russel Coleman, an excellent man, had replaced old Charles Brand so I had written him just before leaving for Canada contrasting the current figures for 1950 with the predictions we had made during the War. They were very close. I pointed out that we had not been called crazy but "pink", which was unpleasant even in those days. Coleman sent me a nice letter. This ended that long and painful controversy over fertilizers.

Again that year we had many visitors from abroad beside Dr. Crowther, including B. E. Butler, A. Løddesøl, E. Gislason, Moormann, Scheys, Pecrot, Zuur, Laq, Huechenhausen, Lonis, and several others.

Now that the Manual was finished I arranged for Dr. Smith to take staff leadership looking to the development of a new and more comprehensive scheme of soil classification. As a start I wanted him to see the current work in Europe. I got approval for him to take a trip in the summer of 1952 and wrote long and detailed letters

about its purpose to Tavernier, Edelman, Aubert, Muir, and others.

I was especially anxious to make it clear that he was coming wholly to learn what they were doing because I knew that western Europeans were getting fed up with American technical advice.

October 6 we had an interesting conference in the Department on rural reading. I gave a paper on Writing scientific and technical books for laymen, which was printed in the "Publishers' Weekly" for December 16.

October 24 to 26 we had special programs at the Plant Industry Station commemorating the 50th anniversary of the organization of the Bureau of Plant Industry. (The Soil Survey was ^{AND} ~~ten~~ years older!) I had a paper for this occasion on Soil classification links research with the farm.

The funniest part of this show was an afternoon session when Brannon was supposed to have talked. He was too busy fighting the Farm Bureau over the "Brannon Plan". So instead the Assistant Secretary, Knox T. Hutchinson addressed us. First of all, whoever wrote his paper didn't gauge the audience because he undertook to

review the scientific findings of the Bureau. But worst of all, his ghost writer used the word "scientist" several times but didn't tell Knox how to pronounce it. Each of the many times he pronounced it science-tist. In the plural it was even funnier. It was quite a strain on the presumed dignified audience.

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Early in November my paper on Soil and land classification, which I had prepared earlier was published in the "Journal of Farm Economics". I don't think Dr. T. W. Schultz ever realized that this was partly triggered by one of his in the Journal that I didn't like. I wrote this paper very carefully in order to define and contrast the words "soil", "kind of soil", and "land". Most of my economics friends get these all mixed up partly because the SCS boys had purposely done so. Thus "soil classification" and "land classification" are two quite different things. The first is a classification of natural objects as they exist regardless of the location and size of their areas. The second is a classification of tracts of land, including location and economic characteristics. Those who fail

This summer and autumn the Secretary's office promoted a publicity program in several bureaus including the ARA on The 5th Plate. This was intended to show that despite our progress in agriculture and surpluses that efforts would need to be redoubled in order to provide for the 20 percent increase in population. I talked with Byron Shaw and others and tried to kill this silly business. I was in favor of promoting research but not for completely the wrong reasons. The economists generally had been objecting to my projections (which turned out to be conservative,) They even told the Secretary that we had in the United States not over ten million acres of soil suitable for crops and now in grass or timber. I told Shaw that if he would be willing to take a ride with me in a car for three weeks I would be glad to show him 150 million. In fact, I had made an estimate in 1937 for the 1938 Yearbook of around 100 million and of course science had advanced a lot since then. But the publication came out in December and made the Department look silly to informed people.

to make this vital distinction are bound to be lead into serious error.

Finally on November 13 Salter moved into the position of Chief of the Soil Conservation Service. Al Moseman became Chief of the Bureau despite the apparent competition. There really never was any competition.

Just after the middle of November I went to Iowa for a conference on land tenure, and also had leisurely visits with Dr. Pierre and his staff. On the way back I had a good visit with Charles Hardin in Chicago.

Salter had taken Ray Heinen with him to the SCS and he and I continued to cooperate on Salter's speeches. I got just a bit suspicious that Salter wasn't taking so firm a hand as he should. He so hated to hurt peoples feelings that it was bound to be his undoing sooner or later.

The day after Christman I went to Philadelphia for the AAAS meetings on the same train that Joan and Robert were taking back to Boston after being with us for Christmas.

We had a rather tedious program for Section O except for

Beeson's talk and one or two good ones in economics.

At this meeting a startling announcement was made about krilium - a soil conditioner that was heralded as the panacea of agriculture. It was supposed to be a bacteria-proof synthetic polysaccharide that would do the work to maintain soil structure of the natural ones produced by microorganisms.

On December 27 a few of us went for a live TV show about the scientific meetings.

One of the sessions had been arranged on fertilizers. It was in terribly bad taste and included several young men in the fertilizer trade. They talked about fertilizers as they would at a meeting of their salesmen. Bradfield and others were thoroughly disgusted. I couldn't miss this chance to get even. So I took the floor and congratulated them warmly and firmly on their splendid program. I went on to say, "Less than ten years ago the fertilizer industry people were working hard to prevent the promotion of fertilizers. They saw little need of them in farming. Nothing could delight me more than this drastic change of policy and to see the representatives

of these same companies publicly urging the manufacture and use of chemical fertilizers. This shows faith in American agriculture and in our country."

Of course these young men didn't know what this was all about and they were highly flattered. But Bradfield did and his face was the color of an American Beauty rose.

When I got home I found myself in the midst of the galley proof of Our garden soils.

1952. Near the end of 1951 I accepted an invitation from the Israel Academy of Sciences to take part in a program on arid lands cooperative between the Academy and UNESCO. I also arranged to make the visit to Scotland that I had had to cancel in 1950.

Shortly after the first of the year I received official notification of my election as vice-president of the AAAS and chairman of Section O. Dr. C. E. Millar was secretary. I wasn't too keen about this job. Several excellent programs for Section O had been arranged during the last three or four years but few agricultural people liked to go to a second professional program between Christmas and New Years. With a great deal of correspondence that ran over the next several months I was able to work out a program for the St. Louis meeting as a ^{my} symposium on Combined resource development with special reference to the Missouri Valley.

Starting early in January, Salter and I had a few conferences about coordination of the Soil Survey to continue the basic program and meet the needs of SCS. It was decided at first Roy Hockensmith and Guy D. Smith would make a joint field trip in the following regions: The ~~S~~outhern States, the Gulf States, the Southwest, and the West. This

was started in January and ended in March. Then while Smith and I were out of the country Hockensmith and W. H. All^away would make similar studies in the Northwest, the Mid-West, and the central and southern Great Plains. These studies were intended to deal strictly with technical and scientific matters, not administration. The reports were to be finished by June.

On January 11 and 12 Dr. John D. Black and I worked together in Knoxville with the agricultural people and the Board. The sulphur fumes from the new steam plants were beginning to create a serious hazard and we impressed upon TVA the importance of finding ways to take out the sulphur. We also went over the problem of records from the "special" farms and made some more suggestions for their improvement. The Board was very suspicious of an agreement about joint work with the Department of Agriculture on soil management. The Board was convinced that the USDA did not want to help TVA but hoped to get power over the authority. Certainly this was not true of Roseman but one had to admit that the record was bad. Recent testimony by

Parker in congressional hearings was obviously intended to belittle the work of TVA in the fertilizer field.

The only possible way any sound agreement could be developed would be for Moseman to take a strong hand. Certainly the approach must first be on ideas and substance, not administration.

On this same trip Dr. Black and I gave a joint public lecture, before the USDA Club, as I recall, on the world food problem and steps to meet it.

After I returned I followed through on the sulphur problem and, with Moseman, on the basis of an agreement for cooperation between the Bureau and TVA and summarized what I had done in a letter to Mr. Gordon Clapp. It turned out that Parker and Jacob had caused so much bitterness with their irresponsible statements about TVA and fertilizers that it was too late to repair the damage.

One important activity that took a considerable part of my time was service on the Personnel Advisory Committee of the Bureau of Plant Industry, Soils, and Agricultural Engineering. I served on this Committee beginning shortly after 1939, was Vice-Chairman of it for several years, and Chairman of it for about the last year and one-half that I was in the Bureau. So far as I have had experience with other agencies, the Bureau had ^{best} the personnel system. There may have been

others as good that I was unfamiliar with. For example, all of the efficiency ratings of all of the people in the entire Bureau were considered by the Committee individually and in groups by grades and class. That is, we considered all of the P-2 soil scientists in relation to one another, all the CAFD-5 secretaries, all of the P-6 plant pathologists, and so on. Where the ratings were out of line they were discussed with the division heads and changed unless the division head brought forward new information. Some were raised and some were lowered. All promotions as well as appointments from outside of the Bureau to grade P-3 and higher were reviewed by the Committee. One division, for example, could not go outside the Bureau to appoint a P-4 plant pathologist if as good or better person in some other part of the organization were at P-3 and wanted to transfer. The operation of the Committee prevented division heads and section heads holding back people from advancement because of the old argument of "lack of funds." I remember many a conference with Dr. John Magness, one of the finest men in the Bureau but very conservative, about raising some of his good people. If it had not been for this Committee some of them might never have been raised and, of course, would have left the Department.

Beginning in the early 1940's the Committee started to develop a new set of special standards for the classification of research scientists. Most of this work was done by Dr. M. A. McCall, Mr. John McAuley, and myself. Our proposals were so contrary to the bureaucratic guidelines of most other agencies in the Department that we made very little progress except for their use by the Committee within the current system that was designed primarily for non-scientific, operational positions. In 1951 Dr. McCall retired and in 1952 I left. But John

McAuley stayed on and kept at it. With the blessing of Dr. Byron Snaw he finally got them approved in the Civil Service Commission despite the objections of the bureaucratic personnel officers of the Department of Agriculture.

The Committee heard appeals from ratings and on other grievances. These were run strictly according to rules of evidence.

The Personnel Committee was only advisory but rarely did the Chief of the Bureau fail to follow its ^C recommendations. There were, of course, some criticisms but far less than I have seen in any other agency. The Director of the Personnel Division and the heads of the sections were very high-class people and devoted their attention strictly to their own field of work. They would not have thought of passing on a quality of the work of the individual scientist themselves. These judgments were made by appropriate panels.

When I came in later to the SCS I found a completely different philosophy, with Personnel people actually assuming that they could judge the promotability of scientific people themselves! A system of rating was developed that had little value because those doing the rating were anonymous and many were even in fields so unrelated to that of the person being rated that they had no basis whatever for judging his basic ability as a scientist. The classification procedure was arbitrary and hierarchical, quite similar to that in the military. Essentially all of the rules were negative whereas in the Bureau nearly all of the rules were positive. Later I had to try to live with this old bureaucratic out-of-date system of classifying and promoting, not people, but "personnel". There is a big difference. Personnel do not get cold or hot; they do not have personal problems; they are all about alike; they are impersonal pegs to be moved around; in short they are not real people.

Late in January I arranged with Dr. Sebrell, Head of the National Institutes of Health, to spend a day with him and his staff going over their system of personnel classification and the work of their advisory committee. John McAuley and LaVerne Richards, our classification officer, went along. We got some good ideas for the improvement of our proposals for the classification of scientists, which finally came to be accepted about ~~ten~~ years later due to John's persistence.

On January 30 the page proof and index of Our garden soils went back to Macmillan. As with every other sizeable work, I was thoroughly sick of it by then.

Dr. Salter and I had also been having some discussions about the need for using more economic principles in the farm planning of SCS. On January 30 I wrote to Dr. Black about it and later he and Sherman Johnson and I discussed it. We were thinking along the lines of making arrangements for this assistance with three to five colleges as a start.

Early in February I made arrangements with Arthur Nelson, then with ECA in Paris, for Harvey Oakes to get a soil survey and a laboratory

started in Turkey. This took a long time but it finally was done and Oakes published a reconnaissance soils map of the country and a road report.

In February I had a letter from Norman Taylor of New Zealand explaining that they were fixing up a box, ~~silver~~ carved by the foremost Mauri carver, C. I. Tuarau, to give to Lucille and me. He had written something about this earlier to Lucille. It was a good thing that he wrote so that I could explain that under the United States Constitution a member of the Government cannot receive anything of value from a foreign government or a foreign prince.

Ray Heinen and I still worked together on Salter's speeches and I had two important ones of my own to do, a long one for Israel and a fairly long one for Ottawa, Canada.

The Work-Planning Conference of the National Cooperative Soil Survey was held in St. Louis, March 24-29. At this meeting we talked quite a bit about our relations with ECA and TVA. I explained the operation of the National Soil and Fertilizer Research Committee that Parker had organized in the hopes of getting more sympathetic support

from the colleges. As I recall now much of our discussion was on problems of classification. I explained the decision to make a new approach toward possibly a new system and the purpose of Dr. Smith's visit to western Europe.

While I was there Lucille called that her father, who had been living with us then, had passed away at the age of 77. My mother was also with us and the two of them went back to Michigan for the funeral. Mother decided to stay there for the rest of the summer. Mr. Reasoner was a fine man but he had been nearly helpless for several weeks.

In April Guy Smith took his first trip to Europe to learn what the Europeans were doing. Our own soil classification was based a great deal on the surface horizons that are destroyed in tillage. Most of our soils in the United States had not been formed long enough to alter their characteristics conspicuously except for the odd place of severe erosion. Many of the soils in Europe had been drastically changed over the many centuries of cultivation, including ~~liming~~ ^{ing} liming, manuring, warping, drainage, and all the other practices. He was scheduled to visit the UK, Netherlands, Belgium, West Germany, France,

On April 7 my old friend L. R. Schoenmann died suddenly. He was taken in the evening while he and his relatively new young wife were looking over the ~~maps~~ ^{maps} and other materials I had sent them to plan a long trip into the western states. This also broke up another scheme I had been working on for about one year. Schoenmann was due to retire about two or three years later. It had been planned that he would move to St. Paul and have an office at the University but on the Soil Survey payroll. Nygard was to fix up a more detailed and up-to-date general soil map of the northern Lake States and Schoenmann was to prepare a monograph of the potentials of the area under combined resource use. Mark Baldwin was to have done a monograph on this area and became too ill to do it. Frank Hayes had started one for the soils of the Great Plains and he died shortly after the outline was firm.

On April 3 Our garden soils was published. When I first opened the book my eye fell on "cool" ashes that should have been "coal" ashes.

In April I had more conferences with Salter about the Soil Survey program and also how to get much more up-to-date information on cropping systems and on fertilizer practices into the SSS program. He had been

out on some visits in the field and was shocked at the backwardness of many of the farm planners in their understanding of modern soil and crop management practices. I also explained what I had been doing with Black and Johnson on economics of farm management.

April 24 Dr. Black came to Washington and we had discussions about getting some simple farm management knowledge into the SCS planning work. Later we were joined by Sherman Johnson and came to an agreement about what might be done. Dr. Black went in and talked to Salter about it. This seed also took a long time to grow, *even a little.*

Near the end of April Salter and I worked out a proposed plan for the coordination of the soil surveys of the Bureau and the Service which I left for ^uAll~~o~~way and Simonson to react to.

On April 30 at 6:50 p.m. I left for Israel. Several of us going to the meeting had received word to be in New York for El Al (Israel airlines) on this date. Besides myself were Dr. and Mrs. L. A. Richards, Professor and Mrs. J. B. Leighly, Professor F. W. Went, and C. W. Thornthwaite. We didn't know how lucky we were that we were transferred to Air France, about which I had heard many bad stories.

(This was the first time I bought air insurance.) The boys in Africa spoke of it as "Air Chance". Yet actually it was the best air crossing of the Atlantic I had had.

The notes and photographs made on this trip, including the visit to Scotland, are included in the Israel Journal, 1952. The program of the Israel Conference is included in the book, Desert Research, 641 pp. Jerusalem, 1953.

We had breakfast the next morning in Santa Maria and changed planes in Paris in the afternoon. Our French stewardess announced the flight as a "non-flight stop" to Lydda, near Tel-Aviv. We arrived there a little after midnight amidst a good deal of confusion. A young lady from the Tourist Bureau had my name and took me through the customs a flying. With her help I got the others through. Thornthwaite was last. He was in a foul mood and had somehow irritated the officers. (In fact he staid in this mood much of the time.) We got to Gazit Hotel in Tel-Aviv about 4:00 a.m. I was so sleepy the next morning that I absent mindedly asked the waiter for bacon in place of eggs. He smiled and brought me sardines.

After breakfast I had a much-interrupted press conference with a reporter who finally reached the right people in Jerusalem and someone came for us. We went in two cars to Jerusalem. That evening and the next day, May 3, we were taken to see the sights. I was looked after especially well by Professor Reinfenberg, Professor of Soils at the University of Jerusalem, whom I had known for some time and had recommended for his present post of full professor.

The next day I went to the University and also visited an interesting kibbutz. When I got back to the hotel that night I discovered that Aubert would be my roommate. What a fortunate coincidence!

On May 5 he and I were taken to see some kibbutzin and we were taken to Rehovot, the principal agricultural experiment station. That evening we attended the initial¹⁰ session of the Desert Conference, which continued until near the end of May 10. I gave a longish paper on, Potentials and problems of arid soils.

The evening of May 10 we loaded into the buses and went to Beersheba and then down into the Negev the next day. For much

of the excursion into this southern area, we were accompanied by some of the toughest-looking soldiers I ever saw with automatic weapons in hand and mounted on jeeps.

On May 12 we went north to Haifa and then took a long trip through Nazareth and out to the lower tip of Lake Tiberias, and back to Haifa.

These buses were about the roughest I had ever travelled in and my back was killing me each night.

May 14 we had a symposium at Haifa and a reception in the evening. On short notice I found that I had to give a serious speech there. The next day Aubert left and I rested a bit and had a nice tour of the old city of Acre. I shall never forget seeing a piece of a Roman column made from Egyptian red granite being used as the door sill into a courtyard. Everyone had left their bit here: The Arabs, the Hebrews, the Romans, the Crusaders, Napoleon, and the British. And of course the Turks were around some too.

In the evening we were taken to the airport. The plane was late. We were awakened to get on the plane about 2:00 a.m. but a fog came in (of all places for fog) and we did not get off until

7:40 a.m., May 16. The plane was terribly crowded. The stewardess was no good at all. The overhead racks were piled with packages that kept falling down. The seats were so uncomfortable that everyone who could walked up and down the aisles. We all piled out at Athens for a bac lunch. The only compensation was the flight over the Alps with a cloudless sky. I could almost reach out and pat the Jungfrau on the nose. We were down in Zurich for tea and in Paris for a late dinner. Fortunately Brigadier Bagnold was along and got me into the Grosvenor Hotel. I don't recall being more tired.

About noon, May 17 I went out to Harpenden chez Muir and talked with my friends for the next two days. I met Guy Smith here for a little while.

May 20 I took an early train to Reading and had a nice chat with Prof. H. D. Kay. In the late afternoon I went on to Oxford and staid with Dr. and Mrs. E. Walter Russell. The fatigue from Israel so upset me that I had gone without food the previous two days and again on that one. Walter and I had some good talks and the next day I went out in the country and visited Sir John and Lady Russell.

In the evening of May 21 I went back to London and took the night train to Aberdeen to visit Dr. A. B. Stewart and his associates. He was then Professor in the University and McArthur was Director of the Macaulay Institute. It was cold here after Israel but I had a hot-water bottle in my bed chez Stewart. The next day we had a most interesting field trip. Then May 24 I looked around^d the University and the Experiment Station. That evening I took the train for London. May 25 I went out to Harpenden and collected the rest of my baggage and was ~~at~~ back at the Victoria air station a little before midnight. But again the plane was late. We actually left the airport at 5:18 a.m. When that plane came in from Israel I don't believe I have ever seen a more sleepy bedraggled group of people get off. We had breakfast in Keflavik and a beautiful view of Greenland on our way to Goose Bay. I finally got home a little after midnight May 28.

On the 2nd of June "Successful Farming" came out with my piece, "Poor" farms - an outdated national luxury as the lead article.

The field reviews by All^aoway and Smith were completed and Hockensmith and All^aoway prepared a joint set of technical recommendations for carrying on the work.

*With this
technical background of*

Agreement Early in June Dr. Salter and I began a series of discussions looking to administrative arrangement for coordination of the soil survey work of both agencies that would be acceptable to the States and give us one National Cooperative Soil Survey that could meet the needs of the SCS and meet the general need ~~for~~ basic soil surveys according to the best scientific standards. We decided against any substantial transfer between SCS and ARA because:

- (1) Transfer from SCS would involve several million dollars and the result might be disturbing to the operation program, or at least appear to be so. Dr. Salter might be subject to very severe criticism from supporters of SCS that "the new Chief was selling the Service down the river." To me it was very important that Dr. Salter remain as Chief of SCS and make it the good program it could be, compared to the past emotional one. Nothing should be allowed to seriously threaten his opportunity to do that. Ray Heinen considered that a transfer of SCS surveys to ARA would seriously injure Salter's position.
- (2) Transfer of the Soil Survey to SCS would weaken the coordination of soil research in ARA and be strongly objected to by the leading soil scientists of the ARA and the Land-Grant Colleges.

Thus we attempted to work out a joint relationship between SCS, ARA, and the States. This I did with consultation with my staff, Roy Hockensmith, and from time to time with Dr. Salter and Mr. Dykes.

All during these conferences I could see that Hockensmith was both weak and worried. He realized his own ~~weak~~ limitations and had been pushed around so long that he was afraid to speak up to Salter, Dykes, or Williams (Chief of Operations, SCS). I spoke to both Salter and Dykes about this and insisted that Hockensmith would have to have more actual control over regional soil scientists if the program worked. On paper, he had technical control over them; but actually he had been mainly a figurehead which they ignored or not as they wished. But he was ~~weak~~ not up to date in soil science.

By the 16th of June I had a fairly good draft that took some more reworking.

June 20 the Ambassador of New Zealand wrote to the Secretary of State asking permission to present me with a carved Maori box as a gift from the New Zealand soil scientists. Later the Embassy told me

that the State Department had no objection and I had a similar letter from the Solicitor of the Department.

Based on arrangements nearly a year earlier I attended the annual meeting of the Agricultural Institute of Canada in Ottawa, June 23 to 26. I gave an address the first day on Using our soils to keep men free. (Later this was a source of amusement to Heinen, Salter, and me. Word got around that Salter had written this one for me "To ease Kellogg into the SCS." As far as I know he never saw it or Heinen either.)

We had some interesting field trips to look at the soils. On the next day I got roped into an unscheduled talk on The soil classification problem. Like all conferences of this sort ⁵ some of the talks were good and some were less than that.

In June Mary Alice became engaged to Fred Hays. He had had a year in Law School at George Washington University after getting his degree at the University of Maryland and had been called for Army duty. In June he had home leave before going to Korea.

Salter and I continued to work on the plans for a unified soil survey. Early in July we developed associated memoranda on Soil Survey ⁶ laboratory needs.

On July 28 we were invited to the New Zealand^{AN} Embassy to receive the much-discussed carved box. It was a beauty. Ambassador Munro made a nice little speech, carefully listened to by two fellows I never saw before from the Department of State. I think they went away, without the box, a bit disappointed. Robert, Joan, and Mary Alice went with us and we had a pleasant luncheon with the Ambassador and his family.

This was a trying time. My staff was worried and so was I, not for myself but for them and for the work. I also drew up a staff paper on publication of soil surveys. Whitlock, in charge of Soil Survey cartographic work, was very worried and told me scandalous things about the cartographic division of SCS which, for all I knew then, could be true.

Moon had been having a series of minor heart attacks and would need to retire.

The International Geographical Union held an International Congress in Washington during the second week in August. I had agreed to serve as chairman of what I knew would be a difficult program to handle. This came on the afternoon of Wednesday, August 13 - A symposium on world food supply. The meeting was very large and was held in the main ballroom of the Hotel Statler. I had gone over the program and we agreed on the rules. None of the main speakers was to have more than a maximum of thirty minutes and the other speakers not more than fifteen minutes. People were not to have more than five minutes for comments from the floor. Too many speakers had been scheduled and unless these time limits were observed, the program could not be concluded until after the dinner hour.

Even before the meeting I had calls from people insisting on fifteen-minute comments. I remember one very articulate screwball who called me and wanted fifteen minutes. I told him what the rules were and that he could have five minutes. He became very abusive and called me "unfair". I told him that I had chaired many meetings and had never been called unfair before.

When the program was to start I got this fellow spotted and had the hotel bouncer sit back of him. Then I got the speakers together and gave them a schedule to which I told them we would adhere. These were speakers notorious to run over. I passed them notes at five minutes before their time and at one minute before. When their time was up I stood up and they sat down.

Then we had the free-for-all discussion. I recognized the man who had called me. He talked exactly five minutes and no more so the bouncer didn't have to work.

The discussion got very hot. A retired professor of geography from Columbia University, who was going to save the world by growing nut trees, Professor Smith, made a rousing attack on the morals of the government of King Farouk of Egypt. It was in terrible taste and about ten people jumped up to be recognized. I saw that we had the makings of a good international riot. I didn't recognize any of them despite their storming. I made a brief statement to the effect that of course honesty and efficiency were important problems in all governments and then I apologized to the Congress and to the audience for Professor Smith's bad taste. This worked and we finished up with

a good discussion about 5:00 p.m.

Professor Smith came around afterwards and told me he had never been put in his place like that before. I said to him, "Perhaps this is your first international meeting. You couldn't have said anything more calculated to hurt the prestige of your profession or our country. You didn't leave me any choice."

SUBJECT: Reorganization of the Soil Survey (1).

Starting early in January, Dr. Salter and I had a few short conferences about the Soil Survey. First it was decided that R. Hockensmith and Dr. G. D. Smith would make a joint study of 4 SCS regions (2, 4, 6, and 7) to see the work of both agencies, determine needs, and appraise the quality of what was being done in terms of these needs. These studies were to deal only with technical matters, not at all with administration. This was completed in March and reports prepared in April. Then R. Hockensmith and Dr. W. H. Allaway made similar studies in SCS regions (1, 3, and 5), while Dr. Smith was in Europe and Dr. Kellogg in Israel. These reports were prepared in May and June.

At the conclusion of these field studies, R. Hockensmith and Dr. Allaway prepared joint recommendations for the carrying out of surveys. These were approved by Dr. Salter and Dr. Kellogg.

With this technical background of agreement, Dr. Salter and Dr. Kellogg began a series of discussions looking to administrative arrangements for coordination of the soil survey work of both agencies that would be acceptable to the States and give us one National Cooperative Soil Survey that could meet the needs of the SCS and meet the general need for basic soil surveys according to the best scientific standards. We decided against any substantial transfer between SCS and ARA because.

(1) Transfer from SCS would involve several million dollars and the result might be disturbing to the operation program, or at least appear to be so. Dr. Salter might be subject to very severe criticism from supporters of SCS that "the new Chief was selling the Service down the river." To me it was very important ^{that} Dr. Salter remain as Chief of SCS and make it the good program it could be, compared to the past emotional one. Nothing should be allowed to seriously threaten his opportunity to do that. Ray Heinen considered that a transfer of SCS surveys to ARA would seriously injure Salter's position.

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Thus we attempted to work out a joint relationship between SCS, ARA, and the States. This I did with consultation with my staff, Roy Hockensmith, and from time to time with Dr. Salter and Mr. Dykes.

All during these conferences I could see that Hockensmith was worried. He realized his own scientific limitations and had been pushed around so long that he was afraid to speak up to Salter, Dykes, or Williams. I spoke to both Salter and Dykes about this and insisted that Hockensmith would have to have more actual control over regional soil scientists if the program worked. On paper, he had technical control over them; but actually he had been mainly a figurehead which they ignored or not as they wished.

Dr. Salter and I gave tentative approval to a document: Suggested program for a National Cooperative Soil Survey and a Unified Soil Survey in the Department of Agriculture, dated July 22, 1952.

Our plan on this date was that Dr. Salter would take copies for discussion with the Regional Directors at their annual policy conference in northern Missedota. Then I would discuss it with him after that. I called the Soil Survey top staff in for a meeting to discuss it after that (August 15 and 16). Then we would seek a conference with the Association of Land Grant Colleges and Universities.

In the meantime copies were made available by me on a confidential basis to Drs. Moseman (Chief of BPISAE), Parker(BPISAE), and Shaw (ARA Administrator).

On August 13 a meeting was held in Dr. Salter's office following his conference with the Regional Directors, who had given approval of the proposal of July 22, 1952. Present were: Salter, Hockensmith, Dykes, Shaw, Barnes, Parker, Moseman, Kellogg, and G. Smith.

Early in this conference Shaw stated that the proposal did not go far enough. There should be one Soil Survey, administratively. Salter stated bluntly that this could only be done within SCS, but that he would not propose it. If it were done he would do his best to have it develop scientifically as well as technically. I said that I would neither suggest it nor oppose it. If I did the former, unkind critics would say I was trying to get SCS money; if the latter, they would say I could not forget Bennett's opposition to me personally because of the several times I had prevented Bennett from getting the Soil Survey. (I had opposed it before because science could not have had a chance under the emotional anti-scientific atmosphere that he had created in SCS).

Shaw also stated that the strictly soil management research should go to BPISAE. He denied it was a trade. But it looked like it to me. I pointed out that months of study had gone into the Soil Survey proposal. The substantive part of the work was all agreed. We were simply discussing administration; and that before we talked about administration of soil management there should be a similar study and agreement on substantive program.

It was agreed that Shaw and Salter would meet alone in the afternoon. In an aside to me both Shaw and Moseman said they would insist that I be in charge of the surveys and research in SCS as Assistant Chief of SCS. I begged them not to make such personal condition. The program should stand on its merits.

I held a conference of my senior staff in my office August 15. Present were Roberts, Ligon, Johnson, Allaway, Ableiter, Simonson, Smith, Whitlock, and also Parker. I explained the July 22 statement, which all had read. Then I threw the bombshell -- Shaw's proposal. The first reaction was very unfavorable.

After lunch we met again. Dr. Shaw spoke at length explaining the greater ease of getting money for the Soil Survey in SCS and that the Research Administration would insist on protection of the scientific work. Moseman spoke in the same vein.

Saturday the senior staff met and broke up into subcommittees to suggest a rewrite of the statement for a "line organization" for the Soil Survey in SCS. We were told that Salter and Shaw would meet with the Secretary about the matter Wednesday afternoon, August 20.

I kept the staff for all the next week, until August 22. We completed a draft statement. The Secretary decided we should move the Soil Survey into SCS Tuesday morning, August 22. Salter spent the entire morning with our staff, explaining the needs of the SCS program and giving assurances that I would be Assistant Chief for Research and Surveys, including soil surveys, other surveys, hydrology, drainage, irrigation, and economics besides general research relations, and that the scientific work would be protected. I gave him a copy of our new draft, realizing that it would not be entirely acceptable.

Of course, during all this time I had long conferences, day and night, with individual staff members. All were deeply shocked at the idea of going into SCS -- of being "SCS men." Smith and Ableiter recovered early. Then Allaway. Roberts and Simonson were, perhaps, the most pessimistic about good scientific work ever being supported in SCS as they left; Roberts, Ligon, and Johnson said they would go along with whatever I thought was sound. If I resigned, they would. If I thought a plan could be worked out, they would try to make it work. Each of the others said substantially the same, although Simonson was still pessimistic. Each one made me promise that if I left for any reason, that I would call or wire at once.

Of course, the basic trouble was the very low scientific standards of most (by no means all) soil scientists in SCS, the undependability of several regional soil scientists and state soil scientists, and the relative smallness for their jobs of the SCS regional directors. The boys expressed faith in Salter, but suppose he died suddenly? I knew that SCS had some good men in the field from the many confidential letters I had received from men who wanted to transfer to the Soil Survey.

I had asked each man to go over the list of SCS men in his area with Simonson and to compile a list of possibilities.

Also I had kept in close touch with Ray Heinen. He gave me copies of a rough policy statement for the men to read. I kept one and went over it very carefully and made many detailed suggestions for its improvement, especially in relation to scientific work and personnel.

Never was I more tired in my life than at the end of the week.

August 25 we met in Salter's office to discuss plans. Present were: Dykes, Williams, Hockensmith, Simonson, Allaway, Salter, and Kellogg. Dr. Salter did most of the talking at a high level of principle. He

wanted to preserve the "line organization" of SCS and the scientific integrity of the Soil Survey. It was lovely, but general. Too many details simply could not be discussed in one conference.

Then Salter left for a week! Don Williams, Hockensmith, Allaway, Simonsin, and myself met in the afternoons of August 25, 27, and 29. Hockensmith came to my office on the other days. We developed many flow charts, all of them partly unsatisfactory.

I had a long private talk with Hockensmith August 26 in which I promised him full cooperation and complete frankness. I told him that some of the Soil Survey staff had questioned his integrity, but that I knew the pressure he had been under from Bennett who (Dykes told me privately) had wanted the early schemes (1942) to fail. He would need to lean over backwards a bit in being frank and honest with the staff. If he did, I was sure he would have their confidence. He promised to do his best. I accepted it in good faith. And he did.

Allaway thought we could develop a good flow chart and program with patience. Simonson remained rather pessimistic.

It seemed wise early to have one Soils Division at the regional office and only one line of technical authority to the state soil scientist. This was contrary to the July 22 statement and Williams was reluctant to approve. It also appeared obvious that Hockensmith was scared -- afraid that he could not hold his own with the Soil Survey staff members-- and wanted a high place in the flow chart, comparable to "Chief of the Soil Survey." That was absurd. Now it seemed to me that Hockensmith was not being frank and not keeping the promise that he made to me. But he did do his best. He felt too much loyalty to unable men.

Over the next week-end I drew two much better flow charts to discuss with Salter September 2. Both provided for the Chief of the Soil Survey to be a member of the top staff of both Operations and Research and Surveys. It must be a man like Smith or Cline if I give it up for Assistant Chief.

Parker dropped in to say that Bradfield had heard of the proposal from Salter. He said that Bradfield was at first strongly opposed to the transfer, but now agreed. Parker said he and Shaw told Pierre about it and that Pierre was opposed. I didn't go into detail with Parker because I could not trust him. While Parker was there a long distance telephone call from Knoxville was announced. Parker left like a shot. It was Joe Moon. He knew the story from Ligon and planned to see me the next Thursday. I hope we can get him back into the Soil Survey for the TVA cooperation.

September 1 Allaway and Simonson came to look over the new charts and discuss them. We all agreed they were better than any yet proposed.

Reorganization of the Soil Survey (2)

On September 2 I had copies made of the flow charts I drew August 31. These were desired in a conference in Salter's office before lunch. Present were: Williams, Salter, Hockensmith, Allaway, Simonson, and Kellogg. In the flow charts (August 31) I proposed the Soil Survey would be intact. It would also serve as soil needs of SCS operations.

Salter suggested a compromise with the old SCS chart. This very much weakened the chart I drew in two ways: (1) Roy Hockensmith would stand in a special position in Operations as well as in Soil Survey. With his kind of loyalty he could become a serious block to cooperation rather than expedition. (2) Salter suggested having a staff of soil management people under the Assistant Chief of Operations for Soil and Plant Management (although Williams and Hockensmith had assured us that there would be none). Such a unit could expand to the detriment of the Interpretation Unit in the Soil Survey. I should prefer to see one unit under Allaway serve both Soil Survey and Operations.

I had very little time alone with Salter. I told him any implication that Hockensmith was or might be superior to the other men on the Soil Survey senior staff was unthinkable. He agreed. Roy would not be in a higher grade, not about GS-14, and never considered for Chief, Soil Survey. I also told him that if I were to handle the other research I should need to spend most of my time on it for the next few months and that a man like Guy Smith should be appointed Chief of the Soil Survey.

Shortly after we got started following lunch, Salter had to leave. Williams, Hockensmith, Simonson, Allaway, and I discussed the need for a description of activities in the regional offices under the "joint interpretation" of chart 2 of August 31. It was agreed that Hockensmith and Allaway would do that the next day. We also discussed personnel for the various places and the possibilities of combined offices at once at Milwaukee and Upper Darby. Several existing and potential State soil scientists were discussed. It was agreed that Templin should not go to Fort Worth and the Roberts should have his choice of Albuquerque, New Mexico, or Portland, Oregon.

Since Salter staid away, the conference broke up at 3:30 p.m. and Simonson, Allaway, and I left for the Station.

On the way back Allaway and Simonson indicated great disappointment with the conference. They felt that the Soil Survey might be so "fenced in" by the arrangement Salter proposed that our staff would have inadequate professional opportunity. I did not take such a dim view; but they talked strongly of resigning then. I was much put out and depressed by this. Why in hell they didn't say what they thought at the conference I didn't know! Obviously they had less faith in Salter than I did. Of course, they didn't know him well. And he had failed to make Parker behave. Neverthe less if the top staff did not go along to SCS I would not. They also objected now to combined offices at Upper Darby and Milwaukee, with SCS men on top, unless we take Spartanburg with Ligon on top also.

When I returned home I had a series of telephone calls:

1. Oliver, General Manager of TVA. He called about Allbaugh for Director of Agricultural Relations. I told him that he was no better than Eric Winters, despite what other members of the staff

had told him. He agreed to wait a month for Starch or for me. He wanted me to come if things here did not work out. Very nice.

2. Eric Winters could not come with Moon Thursday but could come September 10. I told him briefly of the plans here and that we should like to have him in the job as Assistant Chief of SCS Operations for Soil and Plant Management. He said he would be interested and would try to see Williams and Salter September 10. I told him we should like to have Moon joint -- between Soil Survey and TVA -- to handle the programs in the Valley if Ligon went to Spartanburg. He agreed that it was a good idea.

3. Don Williams called, and I told him about Winter's interest and suggested that we still didn't like the flow chart as revised by Salter.

4. I called Allaway and explained Eric's interest. Allaway felt that if Eric would come things would look brighter.

5. I had a long talk with Ray Heinen. He said that Salter felt that Hockensmith needed to be used for a short while in Operations to reduce the fears of the SCS people that "the Soil Survey is moving in to take over." He thought that this feeling was quite strong and that many were afraid of us! Yet he agreed that if my staff would not come the matter could become desperate. He suggested I call Salter. He said also that the meeting with the land-grant people would be postponed to sometime after September 12.

Also he said that Salter vs Shaw and Moseman had a considerable squabble over what research to transfer to BPISAE. The wanted drainage and irrigation but Salter said "no." We shall see. I had thought all along that such a transfer should only follow 2 or 3 months of study and the development of a program. Neither SCS nor BPISAE had any sort of program for research in soil management. Ray said Salter told him "Kellogg was right. We need such a study."

6. I called Salter, told him briefly of our great concern, and asked for a private appointment. He promised to call the time shortly after 9:00 a.m.

7. Finally I reviewed the prospects with Roy Simonson. Reorganization of the Soil Survey (3)

On September 3 Allaway, Simonson, and Kellogg had a frank discussion with Dr. Salter. Allaway and I explained our fears (1) of Hockensmith becoming a block to the flow of soil science from the Soil Survey to Operations, section on Soil and Plant Management. Dr. Salter assured us that he understood Hockensmith's limitation, that his "liaison" position would be only temporary, and that he would not be superior in grade or rank to other staff members of the Soil Survey. He said that soil scientists in Soil and Plant Management of Operations would be approved by Kellogg as well as by Williams. Allaway seemed to be quite reassured. Simonson did not make any specific suggestions. He said generally that he felt the scientific work of the Soil Survey would be neglected. Dr. Salter took this as a reflection of "top management" and said so.

I had a short conference with Williams about the hope to get "Chart I Organization" for Spartanburg with Ligon on top. He agreed to explore it.

After lunch Salter, Mohagen, and I had a preliminary talk about the personnel picture. I insisted on grade GS-14 for the top staff of Soil Survey and regional chiefs of combined offices. Also that State soil scientists of SCS would need to be about 1/2 replaced and the grades raised from GS-11 to GS-12 and to GS-13 for big states with complicated soils and relationships. There were no objections. Then we had a general exploration of all personnel problems. She is very intelligent, but obviously jealous of her "empire."

In the evening at a party for Professor Baskett, Dr. Shaw tried to get me to say that transfer was "the only thing." I told him that no matter how he asked me, I should always say "I neither proposed nor objected, no more."

Reorganization of the Soil Survey (4)

During the morning of September 4, I explained the whole plan, as of the moment to Dr. Guy Smith, who had been away for some ten days. I explained that when, as, and if my duties got too involved to give immediate attention to the Soil Survey, I should expect he might become Chief.

Then I had a long talk with Dr. Alexander, as he had decided to go to SCS with the Soil Survey. I had told him that we were going to have our own laboratory so he would need to decide.

Moon came in during the afternoon and we had a little talk. I explained that I thought he should never go back to his job in TVA, as either Director or Associate Director of Agricultural Relations, after his present sick leave, nor retire yet, but that he should take his old job with the Soil Survey, which was joint with TVA. (Salter and others thought this a good solution to our problem.) During the evening at my home we went over this in detail. I think he was entirely agreed.

During the evening I had a long telephone conversation. While out of the room Moon said to Lucille, she reported later. "The thing is the boys in TVA don't understand me like Doc does."

Before I left the office September 4 Dr. Salter had called. He wanted me to discuss the research transfer with Carleton Barnes and come down to talk to the SCS regional soil scientists the next morning.

In the evening I had a long telephone conversation with Barnes. We discussed SCS research. He told me that Nichols said that "essentially all of the present SCS "research" is soil management." Of course, that was ridiculous and Nichols must have known it.

He was simply trying to "get in good with Shaw." That left Barnes in a difficult spot, made worse by the fact that Nichols' projects and reports were vague and unrevealing. I emphasized very firmly to Barnes that I didn't see how Salter could agree to transfer work dealing primarily with water in terms of stream flow or ground water, engineering, hydrology, or the earth sciences.

Friday, September 5, I had brief early conferences with Salter and Williams about the research deal. Parker and Nichols had in mind to take all "SCS research" except the Soil Survey to BPISAE. I insisted that drainage, irrigation, hydrology, and the like should stay in SCS -- all those things that are primarily earth science, water, or engineering. I suggested the split could be made between "area research" and "experimental plots" better than anywhere else. Then, as an immediate practical matter, one doubted from the record that Parker could either work out a program of research or cooperate with other agencies.

I spoke for 1-1/2 hours with the regional soil scientists, explaining how I thought we should approach our problems and work together. Afterwards, Miss Mohagen, Williams, Allaway, and especially Hockensmith, seemed very pleased. Except for Paschall, and possibly, just possibly, Lewis and Marshall. They were afraid. Apparently the day before they had given Hockensmith a rough time: They had been "soild down the river" and "Kellogg and his bright boys were moving in and taking over completely." But they obviously felt better after my talk.

In the afternoon I explained briefly to Whitlock where I thought we would place him -- that Orsini would be in Operations and the "Soil Map Compilation" would include our facilities and people and part of SCS under him. This would, in turn, be under a soil scientist.

In late afternoon Marlin Cline called from Idaho that he had a wire from Riecken asking him to come in for the Land-Grant Conference. He suggested that Bradfield substitute because of the distance. I agreed, partly for this reason and partly because of his special relation to this Division this summer.

Shaw's office called to say there was a meeting of Salter, Shaw, Moseman, and myself with the "Research Committee of Nine" Saturday morning.

Saturday morning, September 6, Dr. Moseman came by and we went down to this meeting attended by we four from USDA and several experiment station directors (Iowa, Rhode Island, Pennsylvania, California, New Mexico, North Carolina, and perhaps others). The whole matter was explained in broad terms and they were pleased. One said: "Now we have Shaw, Salter, and Kellogg -- three honest men who can sit down and work these things out." Yet I could see again that Shaw was expecting more transfer from SCS than Salter would agree to.

It was agreed that Campbell (Rhode Island), Chairman of ESCOP, would write to the directors September 8 and that Salter and Shaw would send a joint statement September 10, which Heinen and I were to prepare. Dr. Salter announced that Kellogg would handle surveys, research, and research relations for SCS.

I said it would be helpful to have the Soil Survey Committee of the Land-Grant group meet with us to help work out details. Andre of Iowa said, "Will you pay the expense?" I replied that I hadn't thought of it. He said, "Well, if you want their advice you should pay for it." What a narrow point of view! I pointed out that the Land-Grant Committee on Soil Survey had been set up to advise the colleges as well as the Department. Several others agreed and said the Committee "should be independent." But at the end Andre said, "You should pay the expenses." I refrained from making the obvious retort.

At the close one or two remarked, "If Kellogg is going to SCS there is certainly a new day and a better one."

On the way back home Dr. Moseman pressed me to agree that nearly all "SCS research" should be transferred. I agreed on that which dealt experimentally with soil management systems per se, to discover principles for their change and development. But the various soil management systems, accepted as such, used to study water should not.

In the afternoon, A. H. Paschall and Albert Klingebiel were brought to my garden by Dr. Guy Smith. We had a general discussion of the problem. I got only a fair impression of Paschall. He seemed a bit superficial and rather opportunistic. Klingebiel struck me as a sounder more thoughtful man. I thought we should have him in the work by all means.

I talked with Dr. Bradfield by phone at his home urging the importance of an early meeting here with me of the Land-Grant Committee on Soil Survey. He agreed.

I worked out a definition of "soil management research" to guide the selection of "SCS research" to be transferred to BPISAE. I called Dr. Barnes and gave it to him. He agreed to the following:

Note: In retyping inserts, page numbers were changed, hence a few numbers dropped out.

"Soil management research: To study the behavior of combinations of soil treatments and cropping systems, in relation to yields and quality of crops and to soil productivity (including soil fertility, moisture, tilth, and similar characteristics and qualities) under experimental control on specific kinds of soil, in order to establish principles of soil-plant relationships as a partial basis for recommending soil management practices to farm operators according to the kinds of soil on their farms.

Many other kinds of research, not included with soil management research, involve the acceptance and use of recognized soil and plant management systems and principles in order to study other important principles, either through geographic correlation or experimental comparison, such as those principles controlling the behavior of water in watersheds or other areas as related to combinations of climate, relief, geological substrata, kinds of soil, vegetation, and land use. Thus researches designed to establish the engineering and geographic principles for irrigation, drainage, runoff estimation, flood prevention, ground water control, sedimentation, land reclamation and clearing,

soil formation, and the genesis of such landscape features as gullies, sand dunes, alluvial terraces, erosion pavements, slips, seepage spots, and the like, are not included in soil management research."

On September 10 at Shaw's request a statement of the general plan was sent out to the Experiment Station Committee on Policy (ESCOF) of the Land-Grant Association. The chairman of the Committee wired their approval on September 15.

Reorganization of the Soil Survey (5)

Progress was slow after September 6. I had talked again to the regional soil scientists of SCS especially on September 10. As a group, they and senior staff members of the Soil Survey went over the document of July 22 for revisions. I think they felt a little better about the proposal but were still obviously worried as individuals about their place in the ultimate program.

The same day, September 10, I met Dr. Eric Winters and introduced him to Mr. Don Williams. At that time I thought he would be offered the position in Operations as head of the Soil and Plant Management group of divisions.

I also worked with Ray Heinen on the preparation of a statement by Salter and Shaw to go to all the experiment station directors. This supplemented the statement of September 9 sent to them by Director Campbell, Chairman of ESCOP.

A committee had been meeting with Dr. Carleton Barnes to decide on the items to be transferred from SCS research to the soil management division of the Bureau of Plant Industry. This committee consisted of Barnes, Parker, Nichols, and Don Williams. I had been fearful that Parker and Nichols would insist on very large transfers, including research not really soil management and which they would not be qualified to direct. After conversations with Barnes, Moseman, and Williams, I developed a definition for the guidance of the committee. As the committee proceeded, I learned from various conversations with Barnes and Williams that my worst fears were justified -- that Parker was making unreasonable demands for the transfer of work to the Bureau of Plant Industry that had to do primarily with hydrology, sedimentation, and engineering. The evening of September 17th, I had a long telephone conversation with Don Williams. He said that that afternoon he had simply "blown up", and explained why he would never agree to the proposals that Parker had made. Earlier in that day I had talked with both Williams and Barnes about the need for sticking to the definition I had written. Probably the matter won't be settled until Salter and Shaw do it. It was very interesting to me that neither Parker nor Parks had seen fit to discuss any of this with me. At noon September 16, Omer Kelley told me in the lunchroom that he wanted to have a talk. I said, "O.K."

He called my office about 1:00, and I returned the call about 1:15 and said he might come right over. He didn't come, and later called that he was "tied up." Interestingly, he stayed, "tied up!"

On Friday, September 12 the Soil Survey staff went over the list of State soil scientists and rated them according to their adaptability for the work. I gave a copy of this to Roy Hockensmith the following

Tuesday. He was somewhat disturbed that we felt several needed to be replaced. (He did not keep this confidential as he promised to do.)

During the week before September 19, Allaway and Hockensmith had been working on the revised statement, which I went over with them. This was to be sent for comment to the regional soil scientists and regional correlators. It appeared that Hockensmith was now realizing that his future was tied into the Soil Survey, and his attitude was much improved.

Plans were made for a meeting of several of us from Washington with the regional directors in Omaha on October 7 and 8. Plans were also developing for a meeting with the Soil Survey Committee of ESCOP for two days beginning October 1.

September 18 I had a long confidential discussion with Dr. Sterling Hendricks in an attempt to lay the groundwork for a future request to him by Dr. Moseman that he take a position as head of a division of basic soil research in the Bureau, including all the non-Soil Survey work now under Dr. Alexander's supervision plus the laboratory at Ithaca and the one at Riverside. Dr. Moseman and I had been a little afraid that Sterling might not want to do this. As he pointed out, probably Parker would not favor it. Of course the reason was that Sterling had such enormous prestige that Parker was afraid of him; he wouldn't be able to push him around. I also emphasized the importance of Hendricks taking my position as Chairman of the Bureau Personnel Advisory Committee when I left. I thought he would do that too. Thus the groundwork was laid for Moseman to make these moves at the appropriate time.

September 18 I called the Division staff together and gave them an oral informal review of our general plans. Although the aim had been to keep the whole thing confidential until the Secretary's announcement, that turned out to be impossible. So many people had learned a little about it that rather vicious rumors were current in place of real information.

I had talked with Ray Heinen on the 18th and he thought it possible that the Secretary would issue the announcement the next week.

Reorganization of the Soil Survey (6)

On Saturday morning September 20th, I had long telephone conversations with Don Williams and Ray Heinen. They were both worried over the wrangle the committee (Barnes, Parker, Williams, and Nichols) had gotten into over the research to be transferred from SCS to BPISAE. Although up to then all documents and statements had referred to "soil management research," now at that late date, Parker and Shaw

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insisted on "soil, crop, and water management research on farms," which was far broader than Salter had agreed to. Apparently Parker, Parks, Kelley, and Shaw were going to insist, come hell or high water, that the irrigation and drainage work of SCS, including the engineering research, come to BPI. Ray said that Secretary Brannon would be here only the week of September 22-26, and then out until after election. I insisted that we must hold to "soil management research" and work out the adjustments within those terms.

Shaw was apparently very sure that he could get his way -- that the Secretary would be forced to follow his guidance in this matter. I then thought Shaw was getting himself in a position to be badly handled by the Secretary. I felt sure then that Shaw had looked at the whole arrangement as a trade.

I had a long talk with Dr. Moseman and tried to get him to take a more reasonable attitude. It was obvious that Parker and Parks had told him that he should not expect to hold them responsible for the soil management research unless he went all out for the broad statement and big transfer. With examples, I attempted to show him why Dr. Salter could not approve. I stated that it was my opinion that in a row brought to the Secretary, Salter would win, not Shaw; and that the result might even be more drastic -- all soil research to SCS for example. I also reminded him, personally, that Salter and Kellogg were his best friends in the Department and that he should be sure he was supporting a logical position. I hastened to add that I should be the last to suggest that he should support any position that did not believe in but that he should study Parker's proposals very carefully and understand the reasons for them -- whether for research on soils or for "empire building".

September 22 I sent out copies of a revision of the July 22 document, dated September 19, for our senior staff to criticize.

The evening of September 22nd Ray Heinen called and said that a big blow-up might come the next day. H. H. Bennett (probably through Norton), was all excited over the proposed transfer and had gotten some of the leaders in the Association of Soil Conservation District Supervisors stirred up to meet with the Secretary at 10:30 September 23! Just as he and I predicted! This would have been avoided if Shaw and Parker had not tried to change the rules at a late date. He said he had been in touch with Dr. Salter by phone. Waters Davis, the president of the Association was on Salter's side. Ray said he would make a try to get the order signed before the meeting. What a mess!

Later that same evening Don Williams called, also much worried. I reviewed my talk with Dr. Moseman and explained that we in the SCS should have no objection to irrigation and drainage or other water control on research plots under experimental control for the study

of the principles of soil management. He said he had already agreed to that! What a business! Salter had told him a few hours earlier not to agree to the proposals of Shaw and Parker for transfer of SCS irrigation and drainage (engineering) research, nor to the broad language proposed for use in the Secretary's order. Salter had written Shaw an airmail letter which should reach him by September 23.

Interestingly, neither Parker nor any of his associates discussed the matter directly with me. This was a pity because it would have been resolved easily had they done so.

In the afternoon of September 23 Mr. N. A. Back and Mr. Howard L. Cook, assigned to flood control work in the Secretary's Office, called and reviewed the great difficulties they had had in getting reasonable and honest reports from SCS Operations. It was a bad story. Then we had a general discussion of the problem with special regard to research needs.

In the evening of September 23 I called Ray Heinen and he had no bad news on the conference in the Secretary's office that afternoon with representatives of the Association of Soil Conservation District Supervisors objecting to this proposed reorganization. I had mentioned to Dr. Moseman just after lunch that a protest meeting was organized and that it was too bad the order had been held up, which was at the insistence of ARA. Then Moseman called Ray. Ray had told him the story. Moseman urged Ray to call Shaw, which he did. Ray said Shaw made little of it and took the attitude that "SCS was trying to high-pressure him." That angered Heinen. It seemed to me that Shaw was playing high.

In the morning of September 24 Heinen called to say that he had talked with the Secretary early that morning. The conference the day before was not "hot." In fact, Brannon could hardly understand why Heidrich (legislative representative of the Association of Soil Conservation District Supervisors) wanted to see him. He told the Secretary that some people were worried about the transfer, including Dr. Kellogg, but he thought that it was perfectly natural and understandable that Dr. Salter would want Dr. Kellogg with him.

Yet Ray was still uncertain over the signing of the order. Shaw said he would deal only with Salter, so that postponed it.

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Sept

At the end of September we had the dreadful news that Mary Alice' fiance, Fred Hays had been killed in Korea. She was broken up but not quite so badly as his family. Fred was the only son of an old family that had a dairy farm near Barnesville. His father's brother was a practicing physician in Hyattsville, Mr. Hays went so much to pieces that Mary Alice drove him to a cousins home on the Eastern Shore for a few days.

As time went on Mary Alice drew into herself more than usual. The people at Barnesville kept asking her to come out. In the late winter Mary Alice noticed a story in her sorority magazine about a one-year course in administration given jointly by Radcliffe College and the Harvard School of Business. Lucille encouraged this interest in the hopes that she would get away. Finally this worked out.

Although everything was very tense at the office I recall one moment that seemed funny to me then and it does yet. Dr. Hub Allaway was a man of extremes. One day he would be in fine fettle and the next day pessimistic. He would go down and visit Hockensmith and the boys in SCS and come back and tell me that everything would work out

fine. I said, "Hub, this is going to be a very difficult job. Norton and Bennett have been working against us for years. All of that wont wash out in one afternoon."

I recall that he came in to see me one morning after he had been with Roy Hockensmith the day before. He said, "Doc, we've got to be careful about saying that we have to know the name of the soil in order to give a farmer advice. You know, we can tell a farmer a lot about the management of sandy or clayey soils, wet or dry soils, and hilly or level soils," He went on at some length about this.

When he got through I said to him, "There's a lot to what you say, Hub. But how many farmers who will be serious about wanting a farm plan don't already know that much about their soils? We need to tell them these simple things correctly in order to get their confidence but they know them already. Our job, Hub, is to tell them what they don't know and cannot see for themselves, and this takes hard digging and accurate soil classification and interpretation."

Hub looked at me and the brain washing of the day before slid out of him. "You know, Doc," he said, "That is an awfully important point." And he kept muttering it to himself as he left the office.

As Ray Heinen had predicted, the delay in transfer plans caused by failures to agree on what should be transferred from SCS research to the Bureau, gave time for opposition to build up. Apparently Bennett, Norton, Zimmerman, and others had stirred up the officers of the National Association of Soil Conservation Districts (NASCD).

September, 1952.

At the time I would need her competence most Dotty (Burel) told me that she would have to resign. She had been away on maternity leave. The feet of the new-born baby were not entirely right and the physician told her that they should be massaged every two or three hours for a long time. I knew I would have very little possibility of ever having another secretary who could equal Dotty.

Late September 25, Waters Davis, president of the NASCD, came into Washington for conferences on September 26. He was joined by George R. Heidrich, vice-president. Ray Heinen called me a little after 9:00 o'clock the morning of September 26, and explained that Don Williams wanted me to come to the South Building in order to go to lunch with him. I drove down in time for Ray and Don to fill me in. Apparently Bennett and Norton had done a good job of stirring up these people, especially Heidrich.

They had been told that Kellogg opposed conservation, that he opposed the soil conservation districts, and that he would want to turn the SCS over to the State colleges. The basic purpose, of course, was to get after Salter through Kellogg.

Don and I had lunch with them. They asked some pointed questions about capability mapping and about districts. And I asked them some. The conversation was pleasant. Davis was very energetic, conceited, and really quite confused about the purposes and needs of the Department's conservation program. He asked, for example, "How long would it take to complete the land classification of the United States if SCS with all its 13,000 employees were at the one job?" There were others like it. "Why not have Congress pass a law basing wheat or other crop quotas on 'land capability' rather than the history of use?" I then went into some explanation of the land use capability classes and why they were not uniform. I also pointed out that Congress would be exceedingly unlikely to pass legislation of this sort that would have the effect of putting the program completely into the hands of a Federal bureaucracy. I did suggest that it would be possible, considering our knowledge of soils and the other relevant factors, to develop acceptable figures by States and perhaps even by counties, but I thought the distribution within the counties should need to be the responsibility of a local committee only guided by soil maps and other recommendations of Department people.

After lunch Davis gave me a handful of folders -- propaganda for the NASCD -- and said, "Be sure to read all of this before you come to Omaha." (I was astonished to see that less than one third of the districts had paid their full dues to the Association after these several years). In the afternoon, at Davis' request, I attended a conference which he had arranged for some time before with representatives of the Forest Service, PMA, SCS, BLM, Bureau of Reclamation, Fish and Wildlife Service, and others on how to develop a program and statement of conservation needs on both public and private lands. He started out by saying that he had been asked by districts in the West to urge Congress for more funds for this work on public land. He realized that it would be an enormous figure. In his introductory statement he suggested that we should make the calculations

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need to urge Congress for more funds for this work on public lands. He
thought that his organization should present a 20-year plan to
Congress giving the total conservation needs for both public and private
lands. He realized that it would be an enormous figure. In his
introductory statement he suggested that we should make the calculations

on the assumption of "maximum production." Several pointed out that this would be an enormous figure and would result in more production than the country could handle. There was a good deal of confused discussion following this introduction, and I finally made a specific proposal that everyone agreed to. I pointed out that we should need to make some economic assumptions about prices, wages, exports, and population. I suggested that we use the ones already worked out so carefully for 1975 in the Paley report. We could then make two sets of calculations: (1) What would be required to prevent further significant soil depletion, and (2) About what would be required to meet the agricultural requirements in 1975?

Everyone, including Davis, agreed with this and the conference broke up. In explaining the problem I pointed out that if parity prices were suddenly changed to 125, an enormous amount of land would be brought into use through combined drainage, irrigation, and heavy fertilization.

The following weekend I talked with Ray Heinen on the phone and found that these two gentlemen, Davis and Heidrich, had completely misunderstood my remarks (at least claimed they did), and said it was obvious that Kellogg was not in favor of conservation and that he thought the matter could be handled by adjusting parity price! Don Williams was there, and of course assured Ray that I had made a very sound statement. In fact my proposal had saved the conference.

Monday, September 29, I had a dental appointment. While in the dentist's office, Ray Heinen called me that Waters Davis had asked him to send him by airmail special delivery The soils that support us and my principal statement of the last four or five years. I got these together after lunch and sent Ray three books, three bulletins, and about thirty other papers. Ray thought that Davis intended to go through these and perhaps prove that Kellogg was not interested in conservation. I explained to Ray that I would not want Dr. Salter to get into trouble in this, and to be sure that he did not do anything for me that would jeopardize his own position.

Salter returned on Tuesday, September 30, and spent a good deal of his time planning the agenda for the meeting in Omaha the next week with the regional directors of the SCS and the officers of the NASCD.

We, of course, were quite concerned about how our state soil survey cooperators would take the proposed reorganization. Most of them had not been favorable to the SCS surveys, especially Dr. W. H. Pierre of Iowa. So Dr. Salter and I arranged for conferences here with representatives of the states.

The first one was held on Wednesday October 1.

USDA-Land Grant Meeting on Soil Survey, October 1-2, 1952

October 1.

Present: Robert M. Salter, Byron T. Shaw, Donald A. Williams, Charles E. Kellogg, Carlton P. Barnes, Roy D. Hockensmith, Guy Smith, J. K. Ableiter, William H. Allaway, and Roy W. Simonson. For the States: Frank F. Riecken, F. J. Muckenhirn, Richard Bradfield, S. S. Obenshain, C. L. W. Swanson, and R. H. Walker.

Dr. Shaw started the meeting with a brief introduction of its purpose. Dr. Salter outlined the problem of great need for accurate soil surveys in the SCS program. He emphasized that full consideration would be given to the scientific aspects of soil survey and to its many other functions for agencies needing soil surveys.

Kellogg made a very brief statement of the opportunities and turned the discussion over to Don Williams.

Williams explained the general process by which we in the Department had arrived at our present understandings and suggestions. He pointed out that we fully realized that the SCS will need to broaden its outlook and its program. He stressed the need for various kinds of soil survey interpretations -- for work in districts, for the Bureau of Reclamation, and for many other agencies. He realized that some changes in thinking would need to come within the SCS, more in some places than in others. He felt sure that these changes could be made on an objective basis.

Riecken then made a general statement of the problem from the point of view of the States. He emphasized: (1) How is a land-grant college to get proper support and adequate finances and staff to carry on truly cooperative service? (2) Colleges have looked upon the Soil Survey as a research activity and have dealt with it on those terms. At least formerly, the SCS programs have not followed the research method.

Bradfield emphasized: (1) The need for a broad balanced program to satisfy the needs of all farmers. (2) Above all else, the high scientific standing of the Soil Survey as an institution must be maintained. The opportunities for research and high scholarship must be maintained, even though the results do not have a direct bearing on any of the SCS action programs. He said that State people know Salter and Kellogg, and have no fear of their intentions in this respect. (3) Soil survey publications must be kept more current.

Muckenhirn emphasized: (1) The need for full State-USDA cooperation regardless of relative financial contributions. (2) The broad research basis for soil surveys, and recommendation of the wide use of soil surveys. He pointed out that soil classification is not final, and that research is needed as we go along. (3) He also emphasized the

publication problem. We need to reduce the lag, and we also need to be aware of possible conflicts between the needs of the action programs and the requirements of sound scientific scholarship. (4) He was concerned about how the information is given farmers, and the relative roles of Extension Service and the SCS.

Swanson endorsed what the others had said, and expressed concern over the "climate of opinion" within which the work will be done.

Obenshain endorsed the previous statement. He expressed full confidence in Salter and Kellogg, and pointed out that they alone can not keep the program on a proper basis.

Walker reviewed briefly the meeting we had with the Committee of Nine. He was hopeful of the outcome, but emphasized that the program must be fully cooperative regardless of relative financial contributions.

Carleton Barnes emphasized the importance of meeting the general needs outside of agriculture, and that in recent meetings in the Bureau of the Budget concern was expressed as to whether the new program would meet these other needs -- Army, public roads, and so on.

Salter then discussed his familiar ideas of the general relationship between SCS and the land-grant colleges, and pointed out the very large problem of on-farm service that the SCS would give to farmers in districts. He pointed out that we cannot separate the practices required for soil protection and those required for soil improvement. All forms of soil deterioration must be considered. Together we must develop a way to maximize our knowledge about soil use and put it to work on individual farms.

During a short intermission, Dr. Salter had to leave. Then Kellogg outlined in general the tentative plan developed by the staff in Washington for discussion with this committee and with the regional directors. As he presented this, discussion arose on several individual points.

After lunch, Don Williams explained the scheduling of the time of soil scientists and their service to farm planners besides the specific soil mapping. This was followed by a great deal of general discussion and the answers to many questions.

Riecken suggested a sort of State Advisory Committee representing all users of soil surveys to help plan a program within a State. This seemed to be agreeable to everyone.

Between them, Williams and Kellogg explained the general plan within the Service, including a rough flow chart, and how arrangements for work would be made. Kellogg laid emphasis on the Planning Conference

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of the National Cooperative Soil Survey and the proposed Soil Survey conferences by land-grant regions.

The representatives of the States expressed several areas of deep concern, which were discussed by Williams and me as best we could. (1) The great national and international standing of the Soil Survey. (2) The danger that the Soil Survey might become exclusively concerned with the district program. (3) That the classification and mapping work might become too standardized with reduced opportunity for the "free scholar." (4) That State conservationists now have and may have in the future far too much veto power on the soil survey program. (5) That educational work by the Soil Survey itself with farm planners and others may conflict with the State Extension Service. (6) Concern over publication policy. (7) Concern for the necessary training of junior soil scientists before they are assigned to work on their own.

After discussing these points, Kellogg emphasized the following points: (1) Funds used for soil survey activities that are included in the funds voted by the Congress for cooperation with districts must be used for that purpose. There is, of course, administrative leeway in how they are used. (2) Need for the development jointly of a 25-year program to complete the soil survey, with priorities, that would get well ahead of immediate farm planning needs and with a body of argument for support in the Congress and in the State legislatures. (3) Need for more full interpretations for all users on a full cooperative basis at the State level. (4) The conference is discussing plans for new work. It will take a lot of time to review the old work, some of which will need to be written off. (5) Need to publish the significant soil boundaries and the objective soil descriptions. Beyond these two requirements, many methods can be used for presenting and timing interpretations. (6) The strongest argument to depend on with regional directors, State conservationists, and all other people concerned will be good and helpful work. (7) The need for a strong, continuing land-grant committee on soil survey.

On the whole, the meetings that day went well. Bradfield left a bit early. He made an excellent statement endorsing the plan but showing full appreciation of the many difficult problems.

The State people met by themselves in the evening and there was a second joint meeting Thursday, October 2nd.

Present: Donald A. Williams, Roy D. Hockensmith, Charles E. Kellogg, Guy Smith, J. K. Ableiter, Roy W. Simonson, C. P. Barnes,
For the States: S. S. Obenshain, Frank D. Riecken,
R. H. Walker, C. L. W. Swanson, and R. J. Muckenhirn.

Williams proposed that the Committee, before leaving, give Dr. Shaw a brief letter outlining the chief points of concern.

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representatives of the states expressed several areas of concern, which were discussed by Williams and me as best we could.
 (1) The great national and international nature of the Soil Survey.
 (2) The danger that the Soil Survey might become exclusively concerned with the district program.
 (3) That the classification and mapping work might become too standardized with reduced opportunity for the "free scholar."
 (4) That State conservationists now have and may have in the future far too much veto power on the Soil Survey program.
 (5) That educational work by the Soil Survey, itself, with farm leaders and others may conflict with the State Extension Service.
 (6) Concern over publication policy.
 (7) Concern for the necessary maintenance of uniform soil scientists before they are assigned to work on their own.

After discussing these points, Kellond suggested the following points:
 (1) Times used for soil survey activities that are included in the budget voted by the Congress for cooperation with other departments and for the purpose. There is, of course, administrative delay in how they are made.
 (2) Need for the development of a 5-year program to complete the soil survey, with priorities, but would not stand of immediate farm planning needs and which is a basis of argument for support in the Congress and in the State legislatures.
 (3) Need for more full investigations for all users on a local cooperative basis at the State level.
 (4) The conference is discussing plans for new work. It will take a lot of time to review the old work, some of which will need to be written off.
 (5) Need to publish the significant self-connections and the objective self-descriptions. Beyond these two requirements, many studies can be used for presenting and finding interpretations.
 (6) The strongest argument to depend on with regional directors, State conservationists, and all other people concerned is the one on helpful work.
 (7) Need for a strong, continuing land-grant committee on soil survey.

In the whole, the meetings that day went well. Kellond left a bit early. He made an excellent statement endorsing the plan but showing full appreciation of the many difficult problems.

The State people met by themselves in the evening and there was

present: Donald A. Williams, Roy D. Kellond, Charles W. Kellond, Guy Smith, J. K. Kellond, Roy A. Kellond, G. F. Kellond, and the following: J. K. Kellond, Roy A. Kellond, G. F. Kellond, P. H. Walker, G. H. Walker, and R. L. Kellond.

Williams proposed that the Committee, before leaving, give Dr. Shaw a brief letter outlining the chief points of concern.

Muckenhirn pointed out that the Committee had no objection to the consolidation, but had many serious aspects of concern.

(1) We need to provide in the program that the long-time ends of the Soil Survey will be assured. We need to have an increased number of county-wide surveys. I explained again our plans for such surveys and that we hoped for additional funds. Both Riecken and Obenshain insisted that a lot could be done now in that direction without violating the principle of using operational funds to help districts. Muckenhirn said that now the university can get no help from the SCS for surveys. Riecken said SCS now sends soil scientists into county-wide surveys for only a few weeks at a time, and that they cannot work effectively. He insisted that much of the SCS work was inadequate and needed drastic revision. Riecken was skeptical that we could improve the quality of "farm planning soil surveys." All of the State representatives questioned whether policies agreed on in Washington would in fact get down to the regions and the States. At this point Williams explained how the administration operates in SCS, and specifically how Dr. Salter operates.

(2) Muckenhirn also expressed great concern over the scientific work. He feared that the action agency requirements might subordinate it.

(3) Muckenhirn suggested revision of the plan to avoid conflict with the Extension Service. He was worried about the training of people outside the SCS in the use of soil survey information.

(4) The State people suggested emphasis on joint appointments of soil survey party chiefs and others.

(5) They were concerned that the State people were not then informed on what areas were being mapped and were scheduled.

(6) The State people should like more voice in the assignment of the individual soil surveyors according to their competence.

Riecken expressed a general fear that the effect of the program might be to serve the needs of SCS wonderfully, and yet spell the death of the State soil surveys. Kellogg replied that this was a danger, and the most important element in the solution was more understanding and more support for the work in the colleges themselves.

There followed a detailed discussion of many of the points in the tentative outline. Since I entered so actively in this discussion my notes were incomplete. I asked Hockensmith and Simonson to ~~keep~~ detailed notes. We agreed to make several of the changes they proposed and Hockensmith and Simonson were to rewrite these sections the following week.

The conference closed at noon. In the afternoon the State representatives gathered in Mr. Hockensmith's office to develop their letter to Dr. Shaw. A few questions arose that were handled informally.

Dr. F. F. Riecken's letter to Dr. Shaw on behalf of the state representatives simply reiterated the main problems they had brought forth in the discussions. They were especially concerned over maintenance of the scientific and research aspects of the Soil Survey and the principle of full cooperation in the work.

It was of considerable satisfaction to me that during all of these conferences the Experiment Station people had repeated again and again that they had full confidence in Salter and Kellogg but that they were worried about the other officers in SCS.

October 2, Ray told me that Salter was prepared for a real fight if there was any objection to my going into the SCS. I repeated again to Ray that Salter should do nothing that would jeopardize his own position of leadership; that that was much more important than my own place as an individual; that after all I could find lots of work to do. Ray said that Dr. Salter took the position that the success of SCS depended upon my coming there, and he was going to fight it out, come hell or high water. I explained to Ray that I didn't pick scraps, but if when I went to Omaha these people started some kind

act.
of unfair sniping against my work and my record, I ~~shall~~^{could} give them the biggest fight they have ever had and really give them the works - that probably they would lose their tempers while I would not, and the rest of the boys would have some amusement, again come hell or high water.

Lucille and I were quite concerned about Mary Alice who was still on the Eastern Shore with Sprigg Hays, Fred's father.

Dr. Salter was insistent that I go out to the meeting in Omaha with the regional directors of SCS and the officers of NASCD. On the way out Sunday evening October 5, I told him that any citizen had a right to question me about my opinions and get reasonable answers but if they question my integrity as a scientist or as a public servant there could easily be a big row. He laughed and answered only, "You're the doctor."

On the same train were several others from the SCS as well as Dr. Shaw and Dr. Moseman.

We arrived in Omaha Monday evening and started our conferences the next morning. The high officials of the NASCD were already in session. This included the area vice-presidents and directors -- a total of some 25 or 30 people. I suspect that they had already been primed by George Heidrich and Waters Davis, perhaps more especially Davis' assistant Southworth, that Kellogg was suspect. Adjacent to the main table were seated the seven regional directors of SCS, Dr. Shaw, Dr. Moseman, Mr. Dykes, Mr. Heinen, Mr. Williams, and Dr. Kellogg. Dr. Salter took the chair and explained the proposal of combining the Soil Survey in the SCS and the soil management work in the HPI. He was followed by Dr. Shaw, and Dr. Salter said that he understood that some questions had been raised about Dr. Kellogg, and that since he was there, he should speak to them.

I spoke to them as I would to any other serious agricultural audience. I explained that I had been concerned with soil research including soil surveys for a period of some eighteen years in the Department and that our whole effort had been to make as big a contribution as we could to efficient farming, and so on. I laid particular emphasis on the need for the work being both practical and scientific -- that unless it were scientific it could hardly be practical. I emphasized this point especially strongly.

Then I went on to say that I had been, of course, interested also in the other agricultural programs as they related to the use of soil survey and other researches. I told them that I had written several papers along this line (which brought out several smiles, since Southworth had just been through the whole business looking for paragraphs to damage me). I pointed out that I had been interested in two things in those papers.

1. I was concerned to show that through the proper use of science we could solve our major land problems; that it had been easier to dramatize the failures; but that for each dramatic failure I could point out many more quiet successes. I used several illustrations.
2. I also was much concerned that we develop local leadership to take responsibility for the local programs. Of course, I hadn't specifically mentioned soil conservation district supervisors or any other group, since it would not have been appropriate for me to have done so. In this discussion one listener interrupted: "Certainly you don't think there is much danger of a Federal bureaucratic control in the United States." I said, "I am concerned. It has already happened in over half the countries of the world." I continued briefly with my experiences in Italy and Germany before the war and in the Soviet Union since the war.

There were a few questions, which I answered forthrightly.

Shortly after I had started my speech, I had the definite feeling that I was talking to a friendly group that was with me. From subsequent statements I believe that to be true. In other words, the little stink bomb that Mr. Norton or whomever planted with Mr. Heidrich and Dr. Davis

sputtered and went out. During the question period, I had questions from Mr. Alf Larsen, Dr. Dave Doneen, Mr. John Faulkner, and Mr. Bill Richards. They seemed friendly. At the end Mr. Waters Davis was friendly and in good humor. (Insert X at bottom of page)

I had lunch with Mr. Southworth and Mr. Heinen, at which we had a frank discussion about Southworth's reading of my papers. These were sent out by Special Air Mail the week before at a cost of \$20.00. I learned later that he tore sentences out of context and wrote a lot of distorted stuff "to prove" that I was against soil conservation districts for the NASCD officials.

In the afternoon of October 7, we had a meeting with the regional directors of SCS and the top SCS staff in Washington, along with Shaw and Moseman. Salter emphasized the great responsibility that the SCS was assuming in taking on the Soil Survey. He emphasized that the research work of the Soil Survey and its broad program of service must be maintained and strengthened.

Don Williams, with some help from me, explained our flow chart and our tentative proposal. This discussion went on for two or three hours. At the end of it, Dr. Salter went around to each regional director for his comment. Patrick approved. Musser approved. Luker approved, but pointed out that he didn't quite understand the general survey need. McClymonds approved. Merrill said that we needed to consider the soil survey needs in relation to other needs. He said he didn't favor taking money for cooperation with the districts (Public 46 money) for the soil survey. His view seemed rather narrow. He said he hoped that the interim period would be short. He said that many soil maps were not permanent because of increase in erosion. Christ said he was worried about the cost. He made a rather rambling statement. He didn't want to go back to "old conventions." He said soil surveys made in 1900 were out of date, and appeared to suggest that new surveys would be no better. He said he didn't want us to make "nice studies" unless they were really relevant. He said SCS should make the maximum use of money for work to help farmers. On the whole his views seemed to be quite narrow. Buie approved. He thought the move was right, and could be worked out. He said we should be careful not to upset people.

I went to bed shortly after dinner, and didn't hear very many comments. Ray Heinen filled me in by saying that things were going well.

Insert X: While I was speaking one man got up from the table, walked around to the other end and whispered something to Waters Davis. After the meeting Salter told me what he said since he couldn't help but overhear it: "Waters, you lay off this man. We very much need men like him."

We met with the same group again in the morning, and Salter and Shaw discussed the problem of transferring the soil management research to the BPISAE. Salter outlined that the soil and crop management work would go, including that part of the water work dealing intimately with plants. Work relating to sedimentation, hydrology, and the like would remain in SCS. The engineering and hydrology phases of irrigation and drainage would remain in SCS. The work transferred would only be that dealing with water response for crop production. In response to questions, he pointed out that the economics of conservation would "remain" in SCS. The stations at Waco and Hastings would remain in SCS. Some of the work at the Coshocton station would remain and some be transferred. Probably there would be joint projects there. Most of Clyde's division in SCS would remain in SCS. Shaw said that the LaCrosse station would probably remain in SCS, but that some of the work would be BPI.

During the remainder of the afternoon we listened to a good deal of rather disorganized discussion. Several of the regional directors felt that individual research men have had for too little supervision. It was pointed out that the work at Pullman, Pendleton, Tutonia (?), Amarillo, Brownfield, Temple, Ithaca, East Lansing, and elsewhere, would go to BPI, and so would part of Watkinsville.

There was a long discussion of research policy, and it was abundantly clear that Dr. Nichols, the present chief of research in SCS, was exceedingly unpopular with the group. They obviously felt that he had not worked out a program or really tried to meet the problems of Operations. They obviously did not respect him intellectually. This could be both good and bad for me. The group might welcome better attention to their problems but they had an unfriendly feeling toward research people. (This I found out to be true among most of the older men in SCS, easily put down to jealousy.) The suggestions seemed to be in fair agreement that there should be a joint employee at each regional office of the SCS, who would be one-half SCS and one-half BPI. Within the SCS, he would be administratively responsible to the regional director and technically responsible to the chief of research and surveys. I explained the suggestion of a national planning conference on soil management research with the regional conference that involved the land-grant committee.

In the afternoon the same group met and discussed a few miscellaneous problems such as snow surveys, certain personnel classification problems, etc. The prospective appointment of Dr. Winters to head up soil and plant management in Washington under Don Williams was announced confidentially; and also the new head of engineering in Washington, who is presently head of the Civil Engineering Department at Colorado State, D. F. Peterson.

After this session, Don Williams and I talked with Dr. Tom Buie about the possible setup in Spartanburg. This was disappointing because Buie felt that Fuller was a good man (which he wasn't) and that Dr. Ligon was difficult to work with (which he wasn't). We didn't make any progress, although the conversation was friendly and Buie said that he would study the whole matter again.

Nearly the whole group had dinner together, and the atmosphere was friendly indeed. Dr. Salter was obviously relaxed and very nappy about how things had gone. That man had certainly stuck by me so far! The next day in Chicago I told the story to Charles Hardin, and then after that I had a seminar with several of his associates on resource conservation and development.

In the evening on the train coming into Washington, I had a long talk with Ray Heinen. He felt pretty good about the whole business. He felt that the critical men in the field were Patrick and Merrill. He felt then that my relations were perfectly established with Don Williams and Jimmy Dykes. He strongly recommended that, regardless of the pressure for haste, we move very slowly in actually setting things up at the regional offices.

I returned to the office on October 10 and explained all that had taken place to Dr. Simonson, Dr. Smith, and Dr. Allaway.

During the next day or so agreement was reached that the transfer would be effective on November 15. Secretarial memorandum 1318 covering the transfers both ways was issued by Secretary Brannon October 14, 1952 along with a press release of the same date. Salter, Moseman and I each issued separate memoranda to the entire staff of the Soil Survey on the same date. So that on that day, October 14, everybody got the good word and the die was cast.

Obviously I had a great many misgivings. The SCS record on all scientific work had been almost wholly poor. Bennett had worked in the Soil Survey but became a vicious propagandist without intellectual integrity. He was a poor judge of men. None of the regional directors was a first-class man and most of them were quite a bit less than that. They, in turn, were not good judges of men. I had no way of knowing what proportion of the poor men on the regional staffs were their appointments and how many were Bennett's.

Yet I was caught in a trap. Our total funds counting printing, laboratory service and everything were only a bit over \$1,000,000,

which was wholly inadequate for even thinking about completing the soil survey of the United States. (Then too, we had no direct way to get the soil maps used on a wide scale.)

On the other hand in SCS money was abundant. Our problem was to get better men who would actually ^{learn} spend their time at constructive soil survey work. Yet it would be difficult to drop the useless people who couldn't be retrained. Further, much of the time of soil scientists in SCS was wasted in useless training meetings and in conferences that did not need to be held.

In SCS the maps would be used. Once we could replace the so-called soil conservation surveys with proper basic soil surveys, I felt sure that farmers and other land users would demand them. They would be used not only for farm planning but also for planning other land uses that involved engineering interpretations. These were just beginning under cooperation initiated earlier with the Bureau of Public Roads.

In my own mind, however, I had the whole program of "Conservation operations" in view as much as I did the Soil Survey. The farm planning work was not well done. The soil conservationists were poorly informed

use of these large funds could be increased enormously. I knew it would be very slow because of the great retraining that would be required and the backwardness of the regional offices.

Many people have asked me since that time whether I didn't think that this was a better move than the plan not involving transfers that Salter and I had recommended. My only answer has been that the Secretary asked us to try the plan advocated by Shaw. Since we didn't try our original plan I couldn't answer it. By far the biggest problems I saw ahead at that time were (1) extreme bureaucracy and incompetence in the regional offices, and (2) the poor personnel office in the Service. Of course at that moment a great many of the people in SCS, excepting the good soil men in the field, were very suspicious of me. But I didn't believe that would last too long.

After the order came out, except for Dr. Moseman and the Bureau's budget officer, Mr. Victor Beach, my biggest problems were with the Bureau especially Parker and Ed Stephens. I took the position that the funds that had traditionally been used for the soil research in the old Division of Chemistry and Physics, together with the equipment

and space, went with the Soil Survey. Also we wanted to take our own furniture with us that had been purchased with Soil Survey funds. I had to waste many hours in useless conference about this but Moseman and Shaw backed me up.

I didn't want to move down town or have any doings with the research group until after Nichols got out. Nobody had ever been able to work with him and I didn't see how I would be able to.

On October 23 I went to Earlham College in Richmond, Indiana and spoke on the relations between local and state governments in agricultural planning and development. Thorp had left the Soil Survey and William Johnson replaced him as principal soil correlator on October 12.

I took the train from Richmond to Ionia on the 25th. That evening I saw the Saffords and gave a slide talk in the Palo Methodist Church on Israel. The next day I spent mainly at home with Mother. My back was terribly sore. On the 27th I visited several old friends - Ted Sherwood, Jesse Kendall, and others. I recall a long talk with Jim Little, who had been our neighbor to the north when I was a boy.

He seemed to be quite interested in what I had been doing and spoke at length of the cruelty of my father whom he never understood. That evening Mother and I attended a special dinner of some sort at the Methodist Church.

October 28, Mrs Safford took me to Ionia and I got the bus for East Lansing. I had to wait a while for the bus and had a nice long talk with Frank Minier and his wife. His father ran the barber shop in Palo and Frank and I were in high school together.

I got to the Kellogg Center at Michigan State University about 4:30 and attended an Extension banquet that evening. The next day I talked to the Extension people on Poor farms and employment. I had some chats with several of the staff members, cut a platter for the local radio station, had a session with the remaining staff of Schoenmann's Conservation Institute and talked to the Soils seminar on the projected reorganization of the Soil Survey together with a slide lecture on Israel. In the evening I attended the County Agents banquet, which got a bit rough in spots for the initiation of new members.

October 30, I gave another speech on Soil and free men and furnished copies of both speeches to the College. About noon I caught the bus to Detroit and the train to Washington.

On November 5 everybody had the gruesome news that General Eisenhower would be the new President. People had voted partly for a father image and partly against Stevenson because of the accumulation of gripes that the Democrats had collected over a period of twenty years in office. It would be perfectly obvious that the conservative leadership in the colleges would try to reorganize the Department and especially try to get rid of the SCS. They might also try to hurt TVA. But the college people would be for TVA. Much as I distrusted the Republican leadership I couldn't help but get a certain inner satisfaction out of the consternation of the old SCS hands who would have something else to worry about besides me. My biggest concern, however, was the matter of foreign affairs and the great wave of anti-intellectualism epitomized by Senator McCarthy from Wisconsin.

On November 8 I took the train to New York City in order to make a talk about Israel as a part of the United Israel Appeal Campaign. I had taken a cab from the railway station. When the driver came down

the street toward my hotel he was on the opposite side ~~xxxxxx~~ from it. The street was under rep^{air} and very crowded so I told him to let me out and I would work my way across when the light changed at the corner. Because of my sore back I was carrying a cane. My glasses are slightly tinted. As I stood there waiting for the traffic to clear, a second cab came by. Just as it came even with me the driver slammed on his brakes, jumped out of his cab, and came over to help me across the street. He had thought I was blind! Certainly not all cab drivers in New York are as heartless as they are pictured.

The speech the next day went over fairly well but I had a lot of silly questions. In explaining the needs I pointed out some of the difficulties of judgment. Some questioners insisted that strings should be tied to the money in order to spell out just how it would be used so I had to give them some lectures about democracy and responsibility.

My hosts insisted that I return by plane, which turned out to be late but I got home that evening.

The plans for consolidation of the expanded Soil Survey within

SCS went slowly partly because Dr. Salter was away so much. This delay gave me a good deal of trouble at the Bureau. Ed Stephens, the relatively new business manager who succeeded Allanson, became officious and discourteous. People were calling Mrs. Starkey, my business secretary, almost daily with requests and orders that were ridiculous. I had to tell my staff that they were not to act on any suggestion from anybody until it had been cleared with me. This I explained to Moseman and Stephens. Much of the trouble was caused by continued uncertainty about the people who were to transfer from the SCS. We could not move to the South Building until they were out.

Dr. Alexander had the worst difficulty and embarrassment. His decision to come with the Soil Survey had displeased Parker. Since officially Alexander was not to move until the New Year and charts and positions approved, we had no place for him in SCS at the moment. Dr. Moseman (and Dr. Parker for that matter) asked that Dr. Alexander stay on to look after the Atomic Energy work and other research until a successor was appointed. I agreed of course; yet Alexander was not given full consideration in the plans. Questions arose even over

over chemicals, office space, and the like! At the end of November

Parker insisted that ^{Dean}~~here~~ be moved into his place at once! This upset

Alexander because of Dean's complete lack of sympathy with the regulations

of the Atomic Energy Commission and because of his great difficulty

(which was notorious) in working with people. Dr. Alexander told me

that he didn't see how he could continue to act as the Department's

representative on this research if Dean were to be in charge. I called

Dr. Moseman, who told me, "I think it is wrong to put in Dean, but

Parker insists, so what can I do?" This disturbed me very much. Al

should have had more firmness. I talked with him later and suggested

that (1) Dean's responsibility be temporary, (2) apply only to planning

for the future under the reorganization, and (3) not extend to the AEC

work. Moseman agreed and said he would need a complete memo with all

details from Parker and a firm understanding with Dean before agreeing.

I pointed out that when they got a Head for the Division of Basic Soil Research, such a job as prepared for Dean, now held by Alexander, would be wholly unnecessary. Further, putting Dean in such a spot would certainly handicap him in getting a man of A. C. Norman's standing

(he had been proposed) as Head.

I went to the Soil Science meetings in Cincinnati which ran from November 16 to November 20. I talked here at great length with Dr. Pierre and several others about the plans for readjustment. Most of the state people were interested and a bit worried. Many of them were also looking for people. My back was pretty much on a roar during this meeting and I was glad to get home.

On November 27 I went to the University of Chicago for meetings there with Hardin, Schultz, and others. In the afternoon of the 28th I gave a seminar that seemed to go reasonably well. I spent the rest of the day with Hardin and Schultz, and also the forenoon of the next day. That afternoon I took the Capitol Limited back to Washington. Not many people were in the dining car and I had a table by myself. Part way through my dinner the waiter came up and said, "Are you Dr. Kellogg?" I told him I was and he said, "A man at a table back of you says he knows you." I turned around and looked and it was Waters Davis. After we had finished he came back to my roomette and we chatted for awhile. He left and I started to undress. The porter

came and said there was a sick baby in the next car and asked me to come and look at it. I explained to him that I wasn't a medical doctor and didn't know a thing about sick babies. He said, "Yassah" and went away. A few minutes later the pullman conductor came and said, "I don't think you understand the situation, Doctor. The baby in the next car is very ill and we are worried." Finally I got through to him and went to bed.

The next morning I told this story to Waters Davis and asked him never to call me "Dr." again.

Finally I was able to get our flow charts in SCS clarified and improved. J. C. Dykes agreed that the soils work at the regional office should be under an assistant director instead of being tucked down under planning as it had been. At that time we were also thinking of a liaison officer on research working with both SCS and LPI who would also be in this office.

December 1, I sent down functional statements for all the top positions in research and surveys in Washington. But I was having trouble with the editorial and map compilation work. I wanted both

under Ableiter as Assistant Chief, Soil Survey, for Publications. Others wanted part of it in Information and part in Operations. So I just let this simmer. On December 3 I explained Hockensmith's position to him as Assistant Chief for Operations and discussed the functional charts with Salter.

The grades for soil scientists in SCS were fully one grade below what they should have been for the responsibility the men in leadership positions were expected to carry. But a considerable part of the state soil scientists and nearly all of the regional soil scientists were not fully qualified professionally for the grades they did have. Basically, Bennett had not wanted good soil scientists in SCS because they might compete with him. Some good ones were in SCS, but mostly below the levels of leadership in 1952.

All the leaders of the Soil Survey seemed to be coming along with the program pretty well except Simonson and Whitlock. I had treated both of them as fairly as I knew how.

I didn't know exactly why Simonson was so pessimistic compared to the others. Whitlock had got himself crossed up by his attitude that only he knew what to do.

Monday, November 24, the "Wayne Darrow Agricultural Letter" came out with the statement that Republican plans included putting the field work of SCS in the State Extension Services! This was like a bombshell! Waters Davis, president of the ASUD, flew to Washington, then up to see Milton Eisenhower, and out to Salt Lake City to see the new Secretary, Mr. Benson. I had a nice talk with him on the train from Chicago the evening of November 29. He was returning from Salt Lake City and I from a seminar at the University of Chicago.

I didn't think this would happen. I believed the story to have been planted "to stir up the animals" - to scare the SCS into a more reasonable attitude of cooperation with the land-grant colleges. If it had ^o this result, the Soil Survey and other research ^{would} ~~will~~ go along very much better.

It seemed to me ^{then} ~~than~~ to be a large question whether the BPI could organize a reasonable research program in soil management with the present leadership. Certainly Dr. Moseman would do his best but he had very little help. Parker, Parks, and Kelley were more power conscious than soil-problem conscious. A few of us from SCS and BPI

had a meeting in Salter's office to lay out a program for research planning and information to bring about intimate coordination. This could work, in spite of Parker. At this conference the following points were made:

1. The appraisal of research needs should go down to the State level and be cooperative with the State experiment stations. Each State should have a research needs committee cooperative among SCS, EPI, and the experiment stations.
2. At the regional level there should be a comparable committee.
3. For the national summary the research liaison people and division leaders would develop a report.
4. The NASCD would be asked to have both State and national committees to express their views.
5. The position of the soil scientists was not clearly stated at this time.
6. Must arrange for an orderly flow of new research results back to the field workers of SCS in various ways.

Don Williams and some others had raised questions about having the regional soil scientists in GS-14 (they were GS-12), at the level of assistant regional director. We had a meeting with Salter about this and he agreed. It was clearly stated by both Salter and me that this did not suggest that the present incumbents were qualified. We were talking about the grade of the job in the new setting.

December 6 I left Washington for Fort Worth and spent December 8 and 9 there. First Don Williams and I had a conference with Merrill, the Regional Director of SCS. We outlined the plan for a GS-14 soil scientist at the level of Assistant Regional Director, to be responsible for all soils work, including liaison with the BPIB. I had supposed that Dr. Salter had already cleared this organization plan with the regional director. But apparently he hadn't and Merrill said he was much opposed: (1) He thought soils should be under planning to be properly coordinated with the farm planning; (2) he wanted to coordinate the research personally; and (3) he thought that the head soil scientist should not have a higher grade than the regional division leaders under engineering, planning, and soil-and-plant management. (He must have known some very poor soil men in his life!)

I tried to handle these objections mildly. He calmed down a great deal but did not get to the point of expressing approval. He said he would write to Dr. Salter. Unfortunately Don Williams did not support Dr. Salter's decision so strongly as he should have done, especially since Merrill's objections were without substance.

With this bad start we then discussed people. He was very anxious to have Marshall in the top spot and Williams and I agreed (which could have been a serious mistake). We talked about soil correlation. Except for Templin, whom he disliked personally, he knew

none of the others who would be qualified for principal soil correlator. He suggested a bright State soil scientist or survey supervisor in the region now. He had no idea whatever of the problem or of scientific scholarship in soils. I mentioned several and it was agreed that Ulrich and Aandahl would be approached and interviewed by Marshall.

Marshall was called in at the end of the afternoon and told about the plan.

The following forenoon I had a long talk with Marshall about all phases of the work. He agreed that Templin could be very useful. Ultimately, after Oakes gets back from Turkey to handle the eastern part of the region, we should try to get Templin mainly in research. Marshall said that his relations with Templin had been good in recent months. I agreed to try to work out ways for both Aandahl and Ulrich to visit Fort Worth.

I explained our tentative setup and plans in Washington, the National Planning Conference and so on.

Marshall seemed generally agreeable to our plans and ideas. He hoped very much to see the soil scientists upgraded to a level comparable to that of the engineers and others. He thought it would help morale greatly as I did. He thought he could talk Merrill around.

After visits to some of the other Divisions, I left in the afternoon of December 2 for home.

By the time I returned my office was in order in the South Building. We had a time, especially Allaway who was looking after the moving. One day Stephens gave orders not to let us on the place. Fortunately Hub was able to work it out without bringing in Moseman.

December 11 Ray Heinen told me that a Dr. Carl D. Butler was close to Benson and might be in a high place in the Department. I don't remember meeting him but I must have some time. He had told people that he would not state his position on the Soil Conservation Service-Extension proposal "until after I talk with Dr. Kellogg, for whose ideas I have the greatest respect." This got to Heidrick of the NASAD. He went to see Ray and insisted that "they" get hold of me and prompt me about what to say. Ray insisted that I wouldn't work that way: if he or anyone else wanted an interview through the front door, fine, but that "Kellogg would not play under the table." Ray said he got mad, but that Ray told him, "That is the way it is and there is no help for it."

Until a month before Dr. Salter and I were under suspicion for our standing with the States. Now the situation was entirely reversed and we were pointed out as friendly to the States and that there was no need to transfer SCS operations to the Extension Service!

I called Dr. Bradfield that evening. I found that he knew Butler well. He said I had probably met him there at Cornell, and that Butler knew my work well! Bradfield said that he was bright, able, and pleasant. (But as it turned out, he never showed up to talk about the work.)

In December a mountain of work loomed ahead. All the new position descriptions had to be written. Unlike the personnel officers in the Bureau, those in SCS were not at all constructive or helpful. Then too, I had to get acquainted with the research work that I was supposed to be directing. Middleton, who had been an assistant of Nichols, had the project files and was a good bookkeeper. He loathed Nichols and had long since had any initiative thoroughly squelched. To complicate matters, Dr. Salter got the idea of having the divisions that included both operations and research report to both Don Williams and to me. This simply didn't work for either of us and confused the staff no end.

Mr. George Clyde was in charge of the irrigation and drainage research and he had a pretty good staff; and he had an excellent associate, Wayne Criddle. Like me, they both feared that Shaw and Kelley ^{wh} could continue to make attempts to get all SCS research. Clyde hated Omer Kelley and the feeling was mutual. Omer had told people widely that Clyde and his staff were no good and this talk had gotten back to Clyde. (Clyde later became Governor of Utah and Wayne Criddle the chief water authority.) He was very pessimistic

about cooperation with the Bureau and had no use for Salter's administrative scheme.

But as Major Ignatieff used to say, "We kept bashing it out as best we could."

Mary Alice was still living with us and Robert and Joan came for part of the holidays, for the latter part as I recall.

I left Washington on the evening of Christmas Day to chair Section O at the AAAS meetings in St. Louis. I had to attend some meetings on the 26th and the program on Combined resource development with special reference to the Missouri Valley held all day Saturday and all day Sunday. We had an excellent program but, as usual, the crowd was small. If I had not had so many things to do I should have tried to make an arrangement to publish the papers as a group. I think most of them were published somewhere else. I remember that Charles Hardin's paper was quite good. The most I remember about it was a question from the floor: "What is the most important resource of the 17 western states?" He replied, "That's an easy one - 34 senators."

I got home the last day of December just in time to welcome a New Year. I prayed that it would be a better one. (If my prayer was answered the answer was "No".)

1953. Early in January a meeting was held in my old office at the Plant Industry Station following a tour of the Cartographic Division of SCS and of the Cartographic Section of the Soil Survey. Present were Ableiter, Crook, Whitlock, and Kellogg from the Soil Survey; Williams, Mangham, and Orsini from SCS, and M. S. Wright from the Secretary's Office of Plant and Operation.

Williams stressed the importance of getting the new alignment arranged as soon as practicable, including some research on new methods.

I discussed in detail the established cooperation, including differences in the work of the two groups up to now.

Orsini indicated that he had not asked for the job but that he would do the best that he could and Marshall Wright agreed to help in any way he could.

Whitlock was obviously not very happy: He and Crook had some real questions about the ability of the SCS cartographers to handle the detailed soil surveys where accuracy is so important.

I had some of the same fears but later experience proved that Mr. Orsini was an able administrator. He quickly established training programs and within a year or two his staff was not only doing the

work well but also had made improvements. Whitlock didn't stay very long but Crook staid on and made real contributions despite not being entirely satisfied.

Jan 21 1953

On January 21 came the change of administration with Secretary Ezra Taft Benson, a right-wing Mormon and religious fanatic; Under Secretary True D. Morse, an associate of Mr. Doane of the agricultural service of that name - a bit slow and ponderous; and Assistant Secretary J. Earl Coke, formerly of the California Extension Service and a bitter critic of SUS. (Shortly thereafter the Congress allowed the Department several other assistant secretaries.)

Among the many disgraceful things they did was to fire M. L. Wilson, Director of Extension, while he was away on a trip to India. They inspired a vicious story that only a few newspapers would take, suggesting that the reason was because of M. L.'s "Communist sympathies." He was, of course, at retirement age anyway. Perhaps there was no more respected man in the whole field of agriculture.

On February 2 I spoke in Columbus, Ohio to the staff and the graduate students at the University. I spent quite a bit of time

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In the summer of 1939 I had tried to get soil-morphological research started in the Soil Survey. Especially we needed this research in the south^{western} part of the United States because of the enormous changes in climate and vegetation during the past 10 or 15 thousand years. Hugh Bennett and other alarmists were attributing all of the gullies to overgrazing and poor forest management. Actually, most of them were the normal concomitant of the rapid drying of the landscape following the Pleistocene period. We also had other serious problems of geomorphology related to soil genesis and erosion cycles. From a purely practical standpoint it was necessary to be able to distinguish those gullies that were developing because of improper ^{land} use practices from those that were ~~not~~ significantly related to land use.

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Ruhe on the staff and initiated some of the most valuable research in soils that the Department has had.

After he got started we were able to expand the work through cooperation with our own State offices and with the State agricultural experiment stations. Dr. Ruhe organized field training sessions so that the field soil scientists had good opportunities to learn firsthand some of the basic principles of geomorphology.

Curiously, some of this kind of research had been initiated in the old SCS in the middle 30's under C. W. Thornthwaite. They did some good work, especially in the Piedmont of South Carolina, but apparently it was too ^{"scientific"} significant for the leadership who should not ^{have} been expected to be able to understand it, and Thornthwaite and his staff were invited out.

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with our people about a very bad situation for the Cooperative Soil Survey in Ohio which had arisen because of the stupidity of the Dean of Agriculture - Dean Rummel. About three years earlier farmers in the northern part of the state had urged that they have a soil survey in order to plan their drainage and rotations. The Dean did not include it in the budget. The need was great and the pressure continued so the Dean agreed to include it the next year. But at the next hearing he claimed that he forgot about it. This infuriated the farmers and they took their problem to Governor Lausche, who agreed to establish a soil survey in the State Department of Natural Resources, in competition with the University. Then the Dean sent his Experiment Station Director to me and asked that I come out to Ohio and explain to the Governor that he had made a mistake.

I asked the Director, "Can you think of a better formula for me to get in trouble than to volunteer to Governor Lausche unsolicited suggestions about how he should organize his office?"

He said, "I was afraid that you might not want to do it."

"That," I told him, "is the understatement of the year."

This group gotten together in the State Department of Natural Resources was not cooperative. They bought off the junior boys in the University by allotting some funds to them for laboratory work.

I talked with the Dean and this was nearly useless. Both the State Conservationist and the State Soil Scientist were weak. The soil science leadership at the University was weak so not much could be done.

That evening I went on to Chicago and the next morning had a talk with Charles Hardin. I went on to Omaha for the annual meeting of the NASCD and arrived the evening of February 3. Due to some mixup Ray Heinen had not reserved a room for me as he had promised. Somebody then introduced me to Barr Keshler of Iowa. He had a double room at the Athletic Club and offered me the other bed. I went over right away and went to bed.

In the morning of February 4 I got up before him and was dressed and ready to leave when he awoke. I had noticed on his coffee table two or three large envelopes marked "personal" and with E. A. Norton's or H. H. Bennett's home address for return. When he awoke he started

talking to me about Waters Davis for whom he had contempt. He had no use for Dykes and said that he couldn't support Salter if he kept on playing politics with the Mormans. He said Norton was the best man in the SCS but that Salter had him sorting paper clips. Several times I tried to interrupt to tell him who I was but had no chance.

I said, "I don't know anything about these things."

He said, "I know. That's why I'm telling you. Why did they put the Soil Survey in SCS?"

I explained that they obviously had to be combined in either ARA or SCS and that I had not suggested one or the other.

He said, "We were worried about excessive detail."

"This", I explained, "is a myth created by somebody."

I went on to breakfast and then attended the meetings of the committee on education, which was trying to get schools to put in "courses" in conservation. It was not a very inspiring meeting.

The next morning my roommate had been informed about who I was.

He talked more ^{calmly} ~~clearly~~ about the politics of the NASCD and about a crippling accident and other personal history.

After breakfast Ray Heinen told me that he saw my roommate sitting in a corner with Southworth lecturing him. Barr was saying, "But I didn't know who he was, I didn't know who he was." Waters Davis came up to me and said, "Well, let's shake. I understand we're in the same fraternity now."

I went to the program but after lunch was called out by Dr. Salter and asked to take the next train back to work on budgets. ✓

On February 6 on the way back I had a little time for shopping in Chicago and for lunch again with Charles Hardin.

About the middle of February Coke took Don Williams out of SCS and put him in charge of the Agricultural Conservation Program to report to him although the administration of ACP would continue in the Production and Marketing Administration. I never did understand this move.

Also about this time my own Civil Service Grade was changed from 14 to 15.

The most important event of 1953 was a new medicine for my arthritis called butazolidin. My physician hesitated because it had many side reactions, including symptoms of leukemia, skin rash,

nausea, and ulcers. About two people out of five got some one of these. He suggested I try it provided I had blood tests about once a week. Within two weeks it had taken out 80 to 85 percent of the pain. It was almost like being born again. ✓

About February 24 I had worked up all the job descriptions for the senior staff and forwarded them to the Personnel Division.

I spent ^{the} next few days at Muscle Shoals in conference on the TVA fertilizer program. Bradfield, Jacob, Pierre, and several others were there.

When I returned I began endless conferences with personnel people about these job descriptions. For years we had to put up with exceedingly poor personnel service in the SCS. The attitude was wholly negative. The classification work was the poorest. Personnel classification work takes intelligence but is not especially difficult. Yet it had never been good in the SCS. Appraisals for promotion in the career service were not much better. I resented the long hours I had to spend on this work which could have been done for me by an intelligent personnel clerk who understood his job.

Near the end of February or early March came the Eisenhower raid on the Civil Service, which set ~~it~~ ^{research and scholarship} back by about 25 years. All over the government many of the top positions were placed in "Schedule C" - an euphemism for personal or political appointment. In addition, many new exempted positions were created. On top of that, appointments to Civil Service positions GS-14 and higher had to have political clearance, which might take months.

The real reasons for these changes were to permit the appointment of hungry political office seekers and personal friends regardless of competence. The stated reason was absurd. It was claimed that the administration had to have people sympathetic to the program who would carry out orders. Actually this worked just the opposite. Experienced people in Civil Service knew that their job is to carry out policy. If the policy is against their personal ^{consciences} ~~consciences~~ they resign. But where the political appointments are sponsored by a central committee of the party or by a prominent senator or other party member the Secretary's office soon found out that they couldn't be reeducated, disciplined, or fired.

My own position was saved by the bell. The new Secretary of Commerce had tried to fire Dr. Astin, Head of the Bureau of Standards, for not supporting the claims of a man selling a fake additive for old storage batteries. The ^{whole} ~~shole~~ scientific fraternity literally raised hell, although Dr. Astin himself said nothing. His job was saved and the fall guy was the Assistant Secretary of Commerce, Shaffer, who was carrying out orders.

The Administration did make Dr. Salter's position as Chief of SCS exempted through "Schedule C," even though he had offered to leave earlier to take a position as Dean and Director at North Carolina State College. He told me, however, that he saved my position in Civil Service by saying to them, "Kellogg has been in scientific work his whole life. Do you want an 'Astin Case' on your hands?" They did not!

Unhappily, Dr. Salter began to worry a great deal about his status. He didn't have much money saved. Apparently his wife was a free spender and he had been very generous with his children. I honestly think that he had no real cause for worry, ^u b/t he did worry a great deal and the tension was reflected in his circulatory system. Further,

rumors about tearing up the SCS were all about. The Secretary did not take Salter into his confidence and this added greatly to his tension. He felt he would be blamed for anything that went wrong.

In March I learned the rumor that Frank Parker had decided to leave the Department. He had failed to get Salter to appoint him in the SCS and he had discovered that Dr. Moseman was going to run the Bureau of Plant Industry. Thus did AID recruit an 'agriculturist' for India!

We had our National Work-Planning Conference in St. Louis March 9 to 12. By now Dr. Smith, as staff leader, had started to make some progress in looking for a new approach to soil classification. We also discussed some of the problems that were inevitably involved in bringing the two soil surveys together. Not much specific could be done until after both budgets and organization were firmed up under the new administration, which was unfriendly to SCS. All sorts of rumors about reorganization and transfers had gotten out. The college people were still hoping that SCS would be disbanded and the farm planning work turned over to the state extension services.

A curious sidelight on this effort was their failure to get the

support of Charles Hardin whose excellent book had furnished them much of their ammunition. But Charles said to them: "You have all plead for new leadership in the SCS. Now you have it. I don't think that any change should be made until Salter and Kellogg have a fair chance to see what can be done.

I went directly from St. Louis to Minneapolis, Minnesota where I had a commitment to give a paper on The soil conservation job: The public's responsibility before the Minneapolis Farm Forum. Iver Nygard went with me but I staid at the hotel of the Conference. I used this opportunity to visit the people at the College of Agriculture and also the SCS research at the University Hydraulic^C Laboratory.

This visit was very interesting to me. One of the young men in Hydraulics obviously had some questions about my responsibility for research. Yet he was pleasant and showed me some interesting photographs. The drop inlets that had been successful in Wisconsin didn't work in Montana. He showed me slides where erosion had cut around them. "Yet," he said, "the soils are the same."

I asked him, "What do you mean that the soils are the same? They are about as different as ~~any~~ two soils can be."

"No", he replied, "they are alike because they have the same clay content."

Then I explained to him that the Montana soil contained a great deal of gypsum^u that made it highly erodable whereas the other soils in Wisconsin were well structured and firm. I pointed out that he could not design on clay content alone. A structure that would hold on this soil in Montana would be unnecessarily expensive in Wisconsin. He was very appreciative and wondered why he hadn't been told about these differences in soils before. ¹

The afternoon of March 16 I spent at the Farm Forum. My paper came after one by Mr. Doane in the forenoon of March 17.

Ralph Musser and his boys from the Milwaukee Regional Office of SCS were on hand as they had been in 1949 and they were about as much worried. As I came into the room Musser rushed up and asked me, "Have you seen Doane's paper, it's terrible?"

"No," I said, "I didn't know they had been reproduced. But I don't need to read it; he comes before me and I shall hear it."

Mr. Doane's paper was extremely critical of SCS and ACP. He took some of the silliest of Bennett's statements for a field day. He attributed them to a "nationally known soil scientist of the Department of Agriculture."

When the chairman introduced me he told the audience that "the nationally known soil scientist quoted so much by Mr. Doane was not the next speaker." I had taken notes on my paper as Mr. Doane talked and I think most of his critical points were demolished but in a way not to anger him. I recall two points in particular. I turned to him and asked him on the platform, "Why is it, Mr. Doane, that huge government subsidies for irrigation in the West to supply water, which is limiting in the soil there, builds character and self reliance; whereas supplying heavy initial applications of lime and phosphate, to supply the limiting factors on some of our eastern soils, destroys character and develops subservience?" He smiled and the crowd roared.

Then near the end I referred to his attack on price supports. I told him and the audience of my experiences in North Dakota. Good farmers had known that the climate was unstable. They had kept large

reserves of cash in the bank. Then the drought came. Corn was essentially worthless and small grains not much better. The eastern banks called for their money and the local banks failed. Good managers became desperate. Had it not been for special laws they would have lost their farms. "I can say to you, Mr. Doane, that no government can let this happen again and stay in office. For one thing farmers and their families now know that this kind of thing is no longer necessary. Now possibly the administration can be improved. This is what we should be talking about and not the basic idea of preventing these crises."

My paper seemed to be very well received.

After luncheon I sat with the State Conservation Committee and talked with them at some length about the long-time programs within soil conservation districts, which I don't believe they had really considered before.

The next day I spent a few hours at the University of Chicago with Schultz, Hardin, and others on three points: (1) How to appraise technical assistance projects in South America, (2) the work of Resources

For the Future (which never really got off the ground), and (3) plans for a meeting on conservation policy.

After a few busy days in the office I spent April 7 at a conference on food and population at Fordham University in New York City, by invitation. We had an interesting seminar-type discussion. The confirmed Catholics who were also students of demography experienced great difficulty in rationalizing the pronouncements of the Holy Office with the results of their research. One of the young professors at Fordham was a good scholar and might have gone a bit far in his explanation of the problems of growing populations in the newly developing countries as a result of the importation of public health measures from the West without parallel improvements in agriculture.

One of his older colleagues said to him, "God gave man reproductive organs and expected him to use them."

The young scholar-priest turned to him and said angrily, "No such thing. God also gave man a fist but he doesn't expect him to go around punching everyone in the face."

Again I left Washington for a trip to Peoria, Illinois to meet the Mid-Western state experiment station directors. We had our meetings at the USDA Chemical Laboratory. It was interesting to hear them talk about their problems ~~but their problems~~ but their regional research programs were not yet really off the ground. April 15 I went out to the Caterpillar Tractor Company, where the extension directors were meeting and spoke to them on soil conservation along the lines of my last address at Minneapolis.

In the afternoon I went on to Chicago and then over to Des Moines. Frank Mendell, the state conservationist for SCS in Iowa, met me at the morning train and took me to Ames. Relations between Mendell and Dr. Pierre were about as bad as they could be. On the way to Ames Mendell asked me, "Who are you seeing at the College?" I replied, "Especially Dr. Pierre and Dr. Riecken." Then Mendell went on to explain to me that he made it a practice to talk first with the Dean and Director.

I said, "Yes I know that, Mendell, and I wish it weren't so." Dr. Pierre is not sold on this amalgamation of the Soil Survey in the SCS.

I have talked to him about it several times and will again. He has the highest prestige among soil scientists in this whole Mid-Western region. If he goes along the other states will also; if he doesn't we shall have a hell of a time. You, Mendell, can do more to ^{help} ~~help~~ the SCS right now in reassuring Dr. Pierre than can any other state conservationist. (It was very interesting that Mendell took this challenge and did everything he could to overcome the previous difficulties. It took him a long time because Dr. Pierre had a long memory for the ridiculous things that Frank Mendell had supported in the old days.)

After a brief meeting with Dr. Pierre I met with the whole Iowa Soil Survey group. After lunch with Pierre I went back to the meeting and talked with them frankly and at some length about our many technical

~~Today several of us held a session at the University~~

problems and the great need for self study and graduate study for staff improvements.

As I talked with the men it became obvious that Mendell was taking too much responsibility for the scientific side of the work and that the state soil scientist had too little help.

In the evening I had another long talk with Dr. Pierre.

The next morning, April 17, I visited with the people in economic research and with some of the soil science professors. After lunch I spoke to the sophomore students on opportunities in soil science. After that I listened to many criticisms of SCS from the Dean and others. In the evening the state soil scientist drove me to Des Moines and I took the train to Chicago.

All day several of us held a session at the University of Chicago on public policy and soil conservation. Besides Schultz, Hardin, and others from the University I believe that Salter, Dykes, and Williams were there. A young graduate student by the name of David C. Knapp took notes. (Just about ten years later he and I shared an office *with him* *on the Carnegie Study of the Colleges* at the University of Maryland for a while.) We kept going that evening and the next day until after lunch. We took the train for Washington in the afternoon.

After a bit over two days in the office, I left again for Pullman, Washington. On the way out I had lunch with Dr. Hardin in Chicago.

April - 1953

April 27 we started our meeting at Washington State College with the experiment station directors of the Western States. Al Moseman was along and he and I both spoke about the new plans. Nothing very exciting happened.

The next day I spent mostly with the soils people from Idaho and Washington. I also had a good look at the nearby SCS nurseries. I left for home that evening and was back in my office on the first of May.

May 11 and 12 I had conferences at Knoxville, especially with the General Manager. Again I emphasized the great need for having at least a few demonstration farms with complete records to permit precise appraisals of fertilizer efficiency. These should be adjusted to the budget and to represent the major soil associations and sizes and types of farms.

We also discussed the great importance of an immediate emphasis on methods for handling the sulphur fumes from the steam plants. We agreed that if this problem got out of hand TVA would lose much important public support.

We agreed on the need for joint studies between the Department and TVA on the hydrology of watersheds.

We spent much of our time on how to develop a mutually beneficial cooperative program between SCS and TVA on the distribution of fertilizers from the TVA plants for conservation objectives. I had been able to sell this idea to Salter and the regional directors and a letter had gone to TVA from Salter. (Unhappily TVA felt it necessary to allow some of the bureaucratic state extension directors to throttle this excellent idea so it never got off the ground.)

Just about this time in May Lucille became president of the Prince George's General Hospital Guild, which led to a considerable increase in her volunteer work for the community.

During May I worked some more on the position descriptions for my top staff. This was an unnecessarily arduous and time-consuming job. Frank Parker got a last dirty dig at Dr. Alexander and at me by calling the chief classification officer of the Department personnel office, so he said, with the untrue story that Dr. Alexander's work

as Head of the Soil Survey Laboratory would duplicate that of Parker's Division; and that Alexander's job was not a GS-14. Of all the dirty things that Parker did this was about the worst. This muddied the water for Alexander's position so I decided to wait for two or three months. I talked with Dr. Byron Shaw about it and he agreed to do anything that he could, so when the matter came up later I insisted that Dr. Shaw be called and everything was O.K.

As June came on there were increasing rumors of drastic reorganization of the SCS. Nobody knew what the plans were, least of all Dr. Salter. In our budget hearings with Mr. Coke we were asked to make several plans for reorganization including ones that would eliminate all but five regional offices. Gradually it became apparent that Mr. Coke should like to eliminate everything in the SCS except the Soil Survey and the engineering services.^{a/}

a/

Secretary Benson and Assistant Secretary Coke told so many stories to different people that a sub-committee of the Committee on Government Operations of the House of Representatives held hearings on the reorganization of the Soil Conservation Service in early December 1953. These were published with exhibits by the Committee, 215 pp. 1954. It would be hard to sort out the truth. I was present when Coke gave some of the specific orders that he denied later at these hearings as ever having been considered!

8 June 1954

Professor René Tavernier of Belgium spent most of June and July with us in the United States. On that trip he concentrated on soils with fragipans in the eastern part of the country from New York down into Tennessee. It was always a joy to me and to the staff to have him here. He was working very closely with Guy Smith and others to get the European experience integrated with ours in the United States so that the new system would be equally applicable in both continents.

June

During June and July I continued to have an enormous correspondence from my home with my loyal senior staff in the field. They kept me informed of things going on and being said, and I tried to sort out the truth for them. These letters alone would make a book. But certainly it was worth the effort to do what I could to respect their loyalty to me and to keep them from resigning over rumors that had no foundation.

Since Assistant Secretary Coke posed as such a friend of the land-grant colleges Salter and I proposed that \$900,000 of Service funds be budgeted for making contracts for soil survey work by the state experiment stations. We were both anxious to get this done because it would have helped the stations to strengthen their staffs and thus the cooperative program. Coke turned this down flat and no other opportunity ever presented itself.

During this period David R. Gardner was working as my assistant. He was extremely able and pleasant. Part of the time he worked on his thesis which was all that he lacked for completing his work for a

doctor's degree in Public Administration at Harvard. Even at that time he did have occasional periods of illness that he put down to recurrences of the effects of infectious hepatitis that he had had during military service in the coastal areas of Brazil.

On July 1 I got a pleasant order - not to move the principal soil correlators from their locations near land-grant universities to the SCS regional offices.

Mr. J. W. Moon's heart condition did not improve after leaving his job with TVA to return to the Soil Survey. He had frequent heart attacks and was forced to retire in July. I had a wonderful letter from him. We had worked very closely for many years. He was the best man to train bright young men that the Soil Survey ever had. He knew more clearly than most others what the work was really about.

About this same time Dean H. L. Walster retired from North Dakota State College. Fortunately many of my former students were able to organize a very nice affair for him at Fargo that he appreciated very much.

On the face of it things seemed to be moving along with a firm

memorandum about their work to the soil scientists in the field and not too bad a budget. After many difficulties we were starting to make progress on the job of integrating the old SCS Survey with the Soil Survey. The job was especially difficult because of the lack of scholarship among the majority of the SCS soil men and the complete lack of appreciation of scholarship among the regional directors and all the other top people of the old SCS. But we were making some fair starts in each region and conferences were under way in July at each regional office with the top soil scientists of the old Soil Survey.

Then on the morning of July 9 Dr. Salter told me that his doctor had ordered him to leave his job as Chief of the SCS. He said he planned to take Parker's old place and be Director of Soil Research under A. Moseman in the Bureau of Plant Industry, Soils, and Agricultural Engineering. He told me that Bradfield and Bayer were being considered to replace him. It seemed doubtful to me at that time that Bradfield could handle the administrative side of the job.

At a conference with Byron Shaw later that day he told me about the same thing but added that Don Williams might be SCS Chief if Bradfield and Baver turned the job down. He also said that Assistant Secretary Coke wanted to move the Soil Survey and the other SCS research to the Agricultural Research Administration. I told him that Don Williams was a nice man personally but that he seemed to be interested mainly in flood control and appeared to have little knowledge^{of} of soils or agriculture. Yet I had to admit that the appreciation of scholarship was so low in the SCS that the atmosphere for the Soil Survey and other research was now very poor. It would take a strong scientist to revise this atmosphere but that it could be done.

Shaw and I also discussed the possibility of a new scheme for the Soil Survey that would involve a transfer back to the Agricultural Research Administration. But I was aware of the political difficulty of moving all of it back because of H. H. Bennett's influence through the drums of the NASCD. It might be possible to move about half of the total - \$6,000,000 - and leave one-half for farm planning. Shaw said he would like to have a note on it.

Alloway and Simonson prepared a draft that Salter and I took home in the evening. The next morning we discussed it and Salter thought that it might work and thus keep all of the soil research together. I made a few revisions and took it over to Shaw but I still had misgivings about how the appropriations would fare in the future.

In the afternoon Shaw said he discussed it with a group in the Secretary's Office and that a transfer was likely. I raised the question of future financial support. Shaw thought that it would be all right if set up as a separate item in ARA. I strongly urged that the matter be discussed with Congressman Hop^e and Waters Davis in advance.

We also discussed the transfer of the research in the Forest Service to ARA and especially the combining of the hydrology research of the SCS, the Forest Service and the Agricultural Research Administration.

Salter was quite keen about this scheme assuming the following:

- (1) There must be no soil survey left in SCS except rough mapping for farm plans.
- (2) A real effort would be necessary for funds for the Soil Survey, especially for research and publication.
- (3) Good cooperation at state level with state conservationists must be arranged.
- (4) The state soil scientists should be agreeable to SCS, state experiment stations, and the Soil Survey and be technically responsible to the Soil Survey on all matters of soil classification, nomenclature, and the like.

Still I had misgivings about what might happen to the Soil Survey. Salter promised to leave the leadership of basic soil research open to me if I wanted it. But I wasn't thinking so much of myself as of the other men. Then too, the soil scientists on my staff could have a

more beneficial influence - so badly needed - on the farm planning work within the SCS. How I needed a crystal ball!

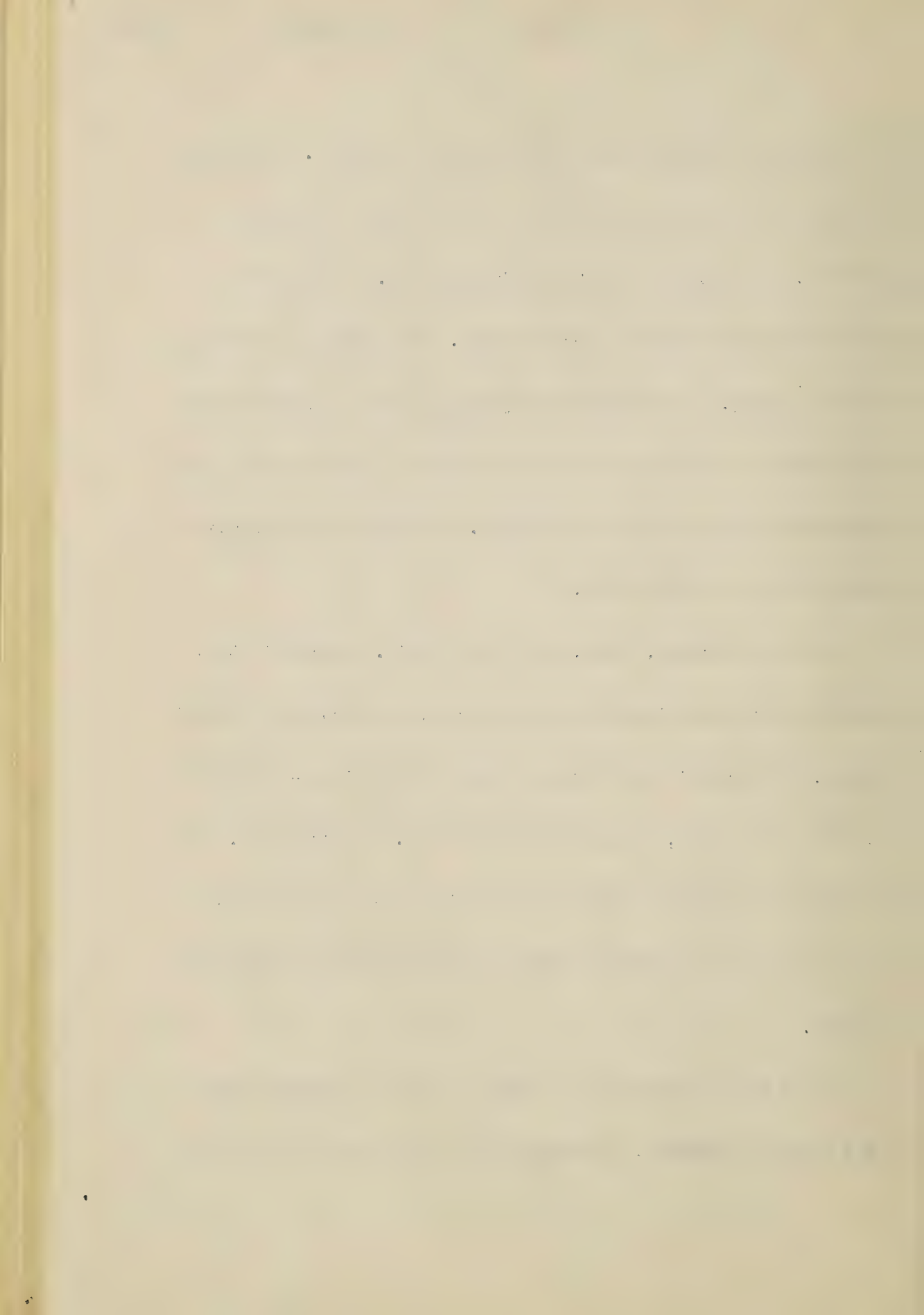
While this was going on Carl Brown and E. A. Norton were juggling the little watersheds provided for in the 1954 budget to get the maximum of congressional advantage. Somehow I lacked confidence in Carl Brown's approach. Howard Cook had deprecated the SCS engineers a great deal but now I was also getting suspicious of his motives. Even after I had arranged a good opportunity for him, he took no advantage of it. (A few months later he transferred to the Corps of Army Engineers.)

I have no idea how it happened but Gladwin Young was brought in from his job on the Missouri Valley Plan to be assistant to Earl Coke. On July 16 he came to me with a list of candidates that Coke had given him for Chief of SCS. Neither of us knew how he had got the names and most of them were out of the question. I mentioned Bayer and Bradfield again but, above all, the new chief should be some prominent soil scientist.

He raised a ^gquestion about the SCS regional offices. I admitted that most of the present directors were not competent for their positions. (Bennett was a very poor judge of men.) But that this fact should not determine the organization. They could be replaced by competent people. I emphasized the enormous handicap of directing the Soil Survey or other research through the present staff of the regional offices or of the state conservationists. He agreed that both should be run as ~~a~~ line organizations.

A bit later Salter, Young, and I had a visit. Young said that Bever had made a poor impression on Coke but was nevertheless offered the job. He turned it down because it was now political - "Schedule C". Then Salter said, "Kellogg should be Chief." Young agreed. I doubted that I wanted it either as a political position and pointed out that the pressure against me from the old Bennett crowd would be terrific.

As the days moved along the rumors got worse but neither Salter nor I were told anything. Everything was so "up in the air" that it



was not possible to plan either Soil Survey or research competently.

In all regions except the southeastern states, meetings were held of the state soil scientists and correlators to develop training programs and ~~des~~^Scriptive legends as required by a memorandum signed by Dr. Salter on June 22. A senior staff man from my office attended each one.

Reports were quite good for the Middle-West and Great Plains and poor for the far West and Northeast.

In the South Dr. Alloway^W went with one or two men from the regional office for meetings in each state. Although in the beginning Dr. Alloway was the most optimistic about the transfer I shall never forget his complete change after this trip. He stood in front of my desk and said, "Doc, you can never make it, never in this world." Except in Kentucky and Tennessee, he had met only resistance and backwardness. From that moment his only thought was to get another job.

On August 7 we got orders from Coke to rejustify all budget items for both fiscal 1954 and 1955. This was a terrific week-end job. Then I got orders to increase (sic) the estimate for increasing the funds for the Soil Survey, which I did from \$1,700,000 to \$2, 500,000. This

would have raised our total from \$5,922,000 to \$8,422,000. It went over August 13.

Then a few days later we got a notice of budget allowances including an \$8,000,000 cut of which \$1,000,000 was to come from the Soil Survey. We also had to work out a federal-state set up without any regional offices, take out about 50 area offices, and reduce the technical staff, especially in biology, agronomy, and the like. But some of the cut had to come out of field offices. After this there was a long period of silence about budgets.

August 24 I went to Boston and saw Robert a few minutes in the evening at the Manger Hotel. At 5:30 in the morning I went up to Exeter in New Hampshire and met with the state conservationists of the Northeast. They were impressed then, I believe, that they had to get down to business on soils training and descriptive legends. We had no fireworks but we had some firm talk.

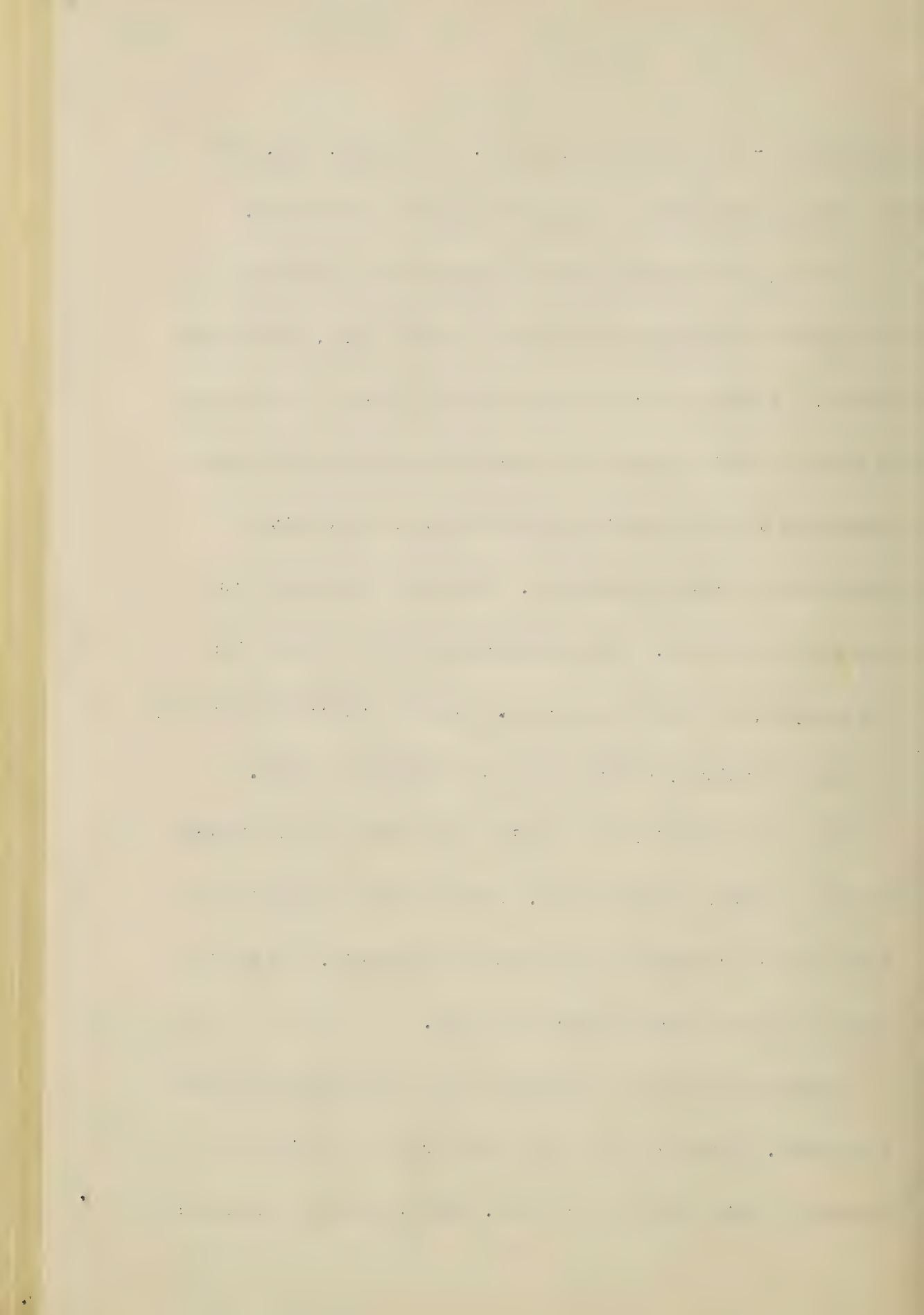
That evening I returned to Boston and spent the evening with Robert and Joan. The next day I had a long session with Dr. Black and he had lunch with Robert and me. In the afternoon I had quite a book

buying spree - the first for many months. Then Robert, Joan, and I had dinner together and I took the evening train to Washington.

I had my only real fuss with the Forest Service about this time. They offered a job one grade too high to Robert Gardner, David's older brother. In order to give him this offer they made him a state agent to supervise "soil and vegetation surveys" financed with state money turned over to the California Experiment Station of the United States Forest Service to administer. I told them that Robert was good but not that good. Before long he made trouble for all of us but especially for the Forest Service. Then they underrated him as much as they had previously overrated him and he finally got fired.

Along about the first of September Lucille went back to Michigan to bring my mother to live with us. Previously she had been in the local hospital at Sheridan to be treated for pneumonia. She was in quite good shape after being here a few days.

September 10 and 11 we had a USDA- State conference on grass and fertilizers. Several of the state leaders brought up their state problems and said what they were doing. Then the research leaders in



the USDA likewise explained their current programs. Assistant Secretary Coke attended an evening session and said that, "From here on the USDA will emphasize research and education rather than other things."

About September 15, Salter, Dykes, and I were called into conference with the budget director and other assistants of Coke and told to take all the cut, except the \$1,000,000 for Soil Survey, from overhead. We reworked the budget as ordered but this would take nearly all the technical staff except that for engineering, soil survey, and flood control. This looked like a move to end the SCS as a national program. Certainly many SCS people had behaved very badly for several years. But Bennett now was gone and gradual changes could have been made. Certainly any one could expect the NASCD to muster enormous support against any such cut in the next session of the Congress.

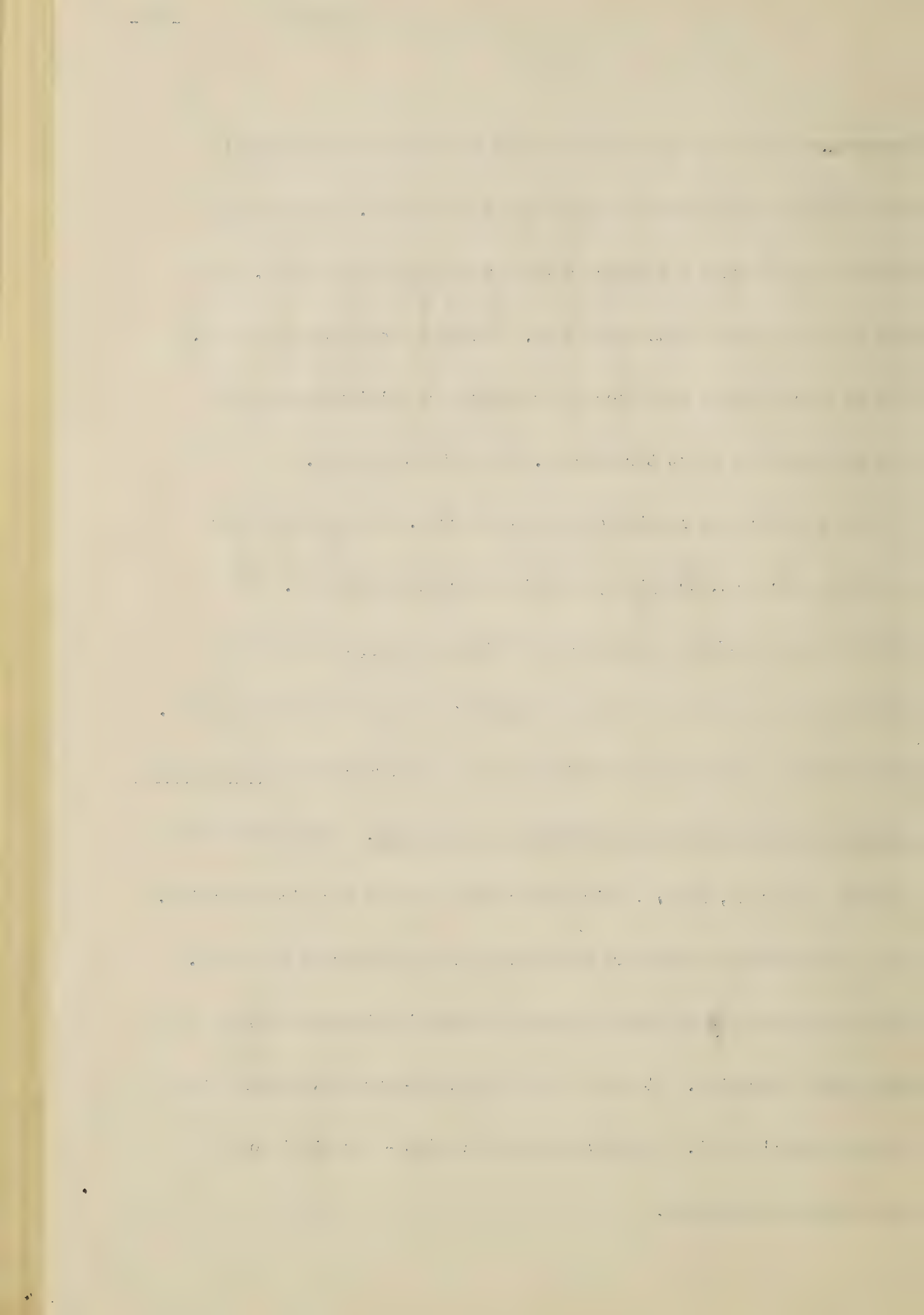
During this last budget hearing I insisted on a meeting with Coke about the Soil Survey and especially the \$800,000 cut in cartography⁴. Salter and I went to see Coke and his staff September 16. I put it up to him very frankly, "Do you want one Soil Survey or two?" I explained our whole plan for the Soil Survey, both objectives and difficulties.

He insisted strongly that none of the people in the old SCS knew anything about soils nor had made soil maps of any use whatsoever. (Apparently he had seen the "soil conservation" maps of the west coast, which did not give the names of the soils and were poorly done indeed.) I explained that some of the maps were poor and failed to map the named kinds of soil, but that 25 percent of the old SCS soil scientists were able to make good basic soil surveys under the general leadership of my top staff. He expressed amazement . I explained that some of them had worked on our cooperative soil surveys as state employees and that a few surveys had been cooperative with SCS. On a little blackboard in his office I drew a map of Iowa and explained the use of the old SCS maps and of the basic soil maps across the State. It seemed to do some good.

Coke was emphatic for one basic soil survey. I explained that the cut of \$800,000 in cartography would ruin it. Our biggest problem was to speed up publication. Then he had to leave. He told the budget officer "to fix it up" and give me the \$800,000 within the current allowances! So later that day at the request of the budget officer I

worked up a series of charges ~~to~~ be made to farmers for individual soil ~~maps~~^{maps} of their farms to make up the total amount. I was simply taking a chance that the Congress would not go along with this. (We had a bit of nervous time about this. The House Committee let it go, but on a motion from the floor the charges were eliminated and the \$800,000 added by about three votes. But this was enough.)

By now word was leaking out about the cuts. The morale of the staff was very low, especially that in the regional offices. The NASCD began to blister Benson in the "Tuesday Letter". These boys were working up a fight to carry to the White House and to the Congress. The Mid-West papers were blistering the new little booklet on Strengthening American agriculture through research and education. (USDA Secretary's Office, August 14, 1953⁵), which never once used the word "conservation." Many congressmen and senators were telling Ike that Benson had to go. Ike gave Benson an assignment to make a speech in Wisconsin but it was purely defensive. At that time I wrongly predicted to myself that Benson wouldn't last. But unhappily he did last - for eight long anti-intellectual years.



We had a terrible blow near the end of September with the sudden death of Dr. Iver Nygard from leukemia.. He was a first-class soil scientist and I had been counting heavily on his help.

At the end of September Mary Alice went to Radcliffe College for a year's special course given by the Harvard Business School.

Stories about cutting out the regional offices of the SCS had gotten into the newspapers. The pressure on Coke and on the White House was terrific. One morning during October I waited at Coke's office to see him. I was afraid that he might weaken. I strongly urged him not to. The regional staffs were poor and if he weakened under pressure he never could fix them. I explained that the only way to fix them was to cut them out. Even if it turned out later that we should have regional offices they could be reconstituted with competent leadership. I told him that I doubted that anyone else in SCS would have the courage to tell him the truth. Most of the staff realized that these offices were unnecessary roadblocks between Washington and the field. He assured me that he would not weaken. Unhappily, as it turned out, Gladwin Young was with him and heard part of this conversation.

On October 12 Dr. Salter called the following into his office to summarize the situation: Dykes, Williams, Kellogg, Heinen, Gaston, Graham, Norton, Hopkins, Carl Brown, Mohagen, and Rule. He undertook to review the situation which worried him no end. He felt that he would be blamed. Salter knew perfectly well that the regional offices should go but he hadn't taken a stand for their removal, as he should have and as he believed, and didn't want to be blamed for their loss.

He started out by saying that he was convinced Earl Coke intended to reduce the SCS in order to build up Extension.

He recalled that early in June there had been a joint meeting in Washington of four state extension directors and four state conservationists (SCS) along with Coke, Salter, Ferguson (the new Republican Extension Director), and perhaps others. Tootel was one of the directors.

Salter quoted him as saying that the two agencies were in conflict; and that much of the work in SCS was educational and should be done by Extension. Another director took the opposite point of view.

Salter reviewed the budget gyrations including ^{my} the alternative without regional offices. He said that he was directed to make cuts

in the field of farm planners and aides but not soil surveyors. He said that on August 24 he had appealed to Under Secretary Morse for reconsideration of the allowances but was refused. He pointed out that by then Coke was devoting all his time to departmental reorganization. It was on August 25 that Coke told Salter that the regional offices would be dropped out and the budget reduced for housekeeping and for technicians ^{above} ~~about~~ the local level.

He reviewed the session of September 2 with Coke (Salter, Dykes, Kellogg, Heinen, and Dorney). We had tried to show the bad effects these proposals would have on technical leadership. Dorney showed that the estimates for housekeeping in Coke's plan ~~were~~ were far too low. Then Salter argued for six regional offices.

On September 10 Dr. Salter sent a strongly-worded memo to Coke presenting this reworked plan under which \$500,000 for housekeeping would be available for technicians.

Coke acknowledged this proposal by telephone but apparently gave it no consideration. Salter concluded that the whole objective of Coke's plan was political - to reduce the SCS in relation to Extension,

not for efficiency nor economy. (I didn't quite agree with that but didn't say so. On the regional offices I thought Coke was honest and right. The old SCS boys had to pay for their years of incompetence and arrogance. It was a crying shame that Salter, who was ill, perhaps more mentally than physically, was taking all of the blame for those many years of Bennett's personal empire.)

Mr. Dykes then took over and explained that we would have a reduced Washington staff, except in the housekeeping functions. This plan suggested an "administrator," in place of a "chief", a deputy administrator, and the state conservationists reporting directly to the administrator. There would be an assistant administrator ~~for~~ for field services and one for management. (Fortunately Coke didn't buy this but insisted on a separate assistant administrator for soil survey. Plant technology was almost put in with it. Considering the staff, thank heavens it wasn't.

The state conservationists would be partly GS-14 and partly GS-13. Each would have a soil conservationist, a conservation engineer, a

state soil scientist, and an administrative officer. The number of assistant state conservationists would vary with the program. The two assistant chief positions held by Norton and Graham were to be eliminated. At this stage all plant science positions were eliminated. There was no plan for handling research, other than the soil survey, but it was to be budgeted. Obviously the other research was to be transferred to Beltsville,

Norton asked how far the people could go "without getting the Schedule C boys in trouble?" (This applied to Salter, Dykes, and Heinen.) He continued, "When this plan becomes known it will be defeated."

Then Salter explained that he was leaving the Service at the order of his physician and going back to Beltsville in charge of soils research. He said that Coke was having trouble to find a successor.

Don Williams said that Coke had told him that after fiscal year 1954 the SCS would no longer service the Agriculture^{al} Conservation Program (ACP) so he supposed that Extension would. He said that he told Coke he should like to return to the SCS since he wasn't

sympathetic to the plans for ACP. (I doubt that he put it that way *to Coke!*)

Salter announced that the regional directors would be in the next day to discuss reorganization. According to present plans the whole departmental reorganization would be explained to the top staff on October 14. After that there was to be a meeting in each region of the state conservationists and the regional staffs. Salter would go to the Northeast and Southeast, Williams to the Southern Plains, Lee Gaston to the Southwest, Carl Brown to the Central Plains, and Dykes to the Cornbelt and the Pacific Northwest.

Already Louis Merrill of the Ft. Worth regional office had resigned. (Which was wonderful news to many.)

At lunch I had a brief talk with Dr. Salter about J. C. Dykes and wondered if I could discuss his qualifications with Earl Coke because of the very unfair rumors about Dykes that I understood were going to Coke. (Actually I believed these were stirred up by E. A. Norton.)

I reflected that evening that the months ahead were going to be very rough. Yet the future for the Soil Survey looked very much brighter since most of the regional directors were not qualified to

deal with that work competently. Salter knew that the regional offices, with the existing staff, were unnecessary, and that these staffs could not be made competent in a reasonable time. When I discussed any incompetent individual with him he always agreed but insisted he was one of a minority. But the trouble was that this minority held many key positions. Don Williams was playing it very cool and quiet. He was doing everything to get into the favor of the SCS staff, the ACP staff, the NASCD, and Mr. Coke.

On October 13 the same staff met again with the regional directors and Salter repeated about what he had said the previous day. He and Dykes both were very critical of Coke's intentions. They simply would not recognize the great body of opinion in the SCS against these offices. Don Williams was critical also but he mixed it with a kind of shrewd shyness and cynicism. Salter said that the meeting the previous June of state conservationists and extension directors, "would have been good if Coke had been open minded."

Salter then said, "We will discuss the steps to be taken if we have to follow the damn thing." (Coke's plan.)

About 11 o'clock Rule distributed copies of the new plan for the Department that split the Bureau of Agricultural Economics and eliminated the research bureaus within the ARA, which then became the Agricultural Research Service (ARS).

At 11:30 we went to the auditorium and heard Benson and especially Coke explain the plan which was not yet completely finalized. Coke did indicate on a chart for SCS an Assistant Administrator for Soil Survey not mixed up with anything else. As I left I went by Moseman and Quisenberry. Both had tears in their eyes. (This part of the reorganization was pure Shaw - not Coke. Shaw had tried to get this done much earlier. It turned out to be a bad move.)

We went back to the conference but Dykes stuck to his special chart without the assistant administrator for Soil Survey. There was much talk about "If Benson's plan sticks." Williams said that we cannot hope to save the regional offices but we should try to save the research and more "top-level" technical staff.

I was annoyed and amazed to see Bill Southworth, an employee of the NASCD, permitted to sit in this highly confidential conference

and take all the handouts. I knew this would make trouble. (And it did.)

October 14 I discovered that Mr. Coke had seriously misquoted me to Waters Davis so I wrote Waters a letter denying that I had ever said that less than 25 percent of the soil surveyor's time in the old SCS was spent in soil mapping. I explained what I did say, all in the presence of Dr. Salter and others.

On October 14 I went with Dr. Salter for a slow, pro forma defense of our budget at the Bureau of the Budget. Obviously the examiners were not enthusiastic about the cuts. (No examiner likes to be overruled by the Congress and they knew that they would be on this.) But all had their orders.

In the afternoon we went over in more detail with the regional directors what had to be done in the months ahead. At 4:30 p.m. the whole staff went to the auditorium to hear Coke explain the reorganization. He again used his chart for Soil Survey - not Dykes'. When we went out Heinen said, "I hope Coke has the other charts more accurate than those for SCS. The budget cuts makes his impossible."

I explained to Ray that the budget had nothing to do with the organization of the Soil Survey. I felt there might be some kind of intrigue.

(Ray Heinen and I had worked very closely up to this point and gradually less closely in the years that followed for some reason that I have never known.)

During the next two weeks I was acting administrator a good deal of the time while the others were away for the meetings in the regional offices. All of the details of SCS were broadcast by the NASCD to the newspapers and to members of Congress. A reporter got Coke out of bed for the morning papers. He called me and gave me a blistering lecture on the telephone. I told him that I didn't know how it got out. I called Williams, Salter, and Dykes and they all swore they didn't know. A little later I made my peace with Coke. At the hearing in the House of Representatives on December 3 and 4, 1953, Southworth admitted attending the meeting, taking the confidential materials, and giving them circulation.

Salter had asked me also to approach Karl Quisenberry for the job of heading up Plant Technology after we had had Eric Winters in for

a conference ^{about} it. Karl was awfully mad at Shaw and Coke but not quite mad enough to transfer. Also I had the job of negotiating with ARS on the details of the transfer of research. This was a big job because part of the boys were one-half research and one-half operations and because some didn't want to transfer. In the past most of the travel funds had been from operations so I got Salter to agree to transfer some \$50,000 or so that we were not required to transfer so that the work could go on.

October 22 I wrote a personal letter to Dr. Pierre summarizing briefly how ~~things~~ were going and pointing out Hub Alloway's unhappy attitude. I was really asking Pierre to talk to Hub about it to see if he couldn't convince him of his great opportunity in the reorganized Soil Survey. Ever since his visit to state offices in the southern states, Hub was trying to get back to Beltsville. He always had moved about a great deal, however. To him, the grass was best just over the fence.

Salter kept explaining that up until November 1 Benson had planned to appoint a South Carolina businessman heading a seed company as

Administrator of the SCS. Coke had not got any top-level soil scientist to accept a Schedule C job. At the last minute Salter insisted that he talked the Secretary into naming Don Williams.

So the announcement came out November 2 that Williams would become the acting administrator of SCS and Salter would transfer to ARS in charge of soil research. (Soon Williams had his political clearance and the "acting" was dropped.)

I saw Williams with Salter for a few minutes after lunch. We told him about the plan for Quisenberry. He said, "We should not make any commitments until after the organizationa chart is firmed up." This was no answer since it already was firm. Don talked well at the staff conference later except for his amazing statement, "I have done nothing ~~whatsoever~~ whatsoever to get this appointment." But he sure did and very shrewdly.

The feelings ^gagainst the elimination of the regional offices and the budget cuts (plant technology had been put back already) were running very high. Many congressmen had spoken strongly about it. This was the reason that the Secretary put the plan into effect at

once. It seemed obvious that there would be an investigation and that the cuts would be restored.

Coke appointed Gladwin Young as Deputy Administrator, which moved Dykes out. Williams, who was an engineer, and Young with a little economic training at Purdue, would run the nation's largest soil program from Schedule C jobs. Williams told me that he had a big job to keep Dykes at all. But he was made Assistant Administrator for field services. Still so long as Benson remained he could not attend budget hearings.

We had a slight calm after the explosion of November 2 with Salter leaving^g and Williams coming in. On November 3 there was much activity in the Administrator's Office but my opinions were neither sought for nor put forward. Late in the afternoon Williams asked me down to discuss the qualifications of Dr. Karl Quisenberry to head Plant Technology.

Williams told me that he wanted me to stay as Assistant Administrator for Soil Survey. I told him that I was willing to leave if he should

rather have someone else but he insisted that I stay. It was apparent that I would have cartography but nothing was said about plant technology or research liaison, for which I was thankful. I had all I could hope to do with the Soil Survey. Williams spoke vaguely of readjustments and the like. He said he ~~was~~ assured that he would be "acting" for only a short time and would then be given the full rank. He further said that he felt sure he could pick exactly whom he wanted for his staff. Obviously his chief concern was simple : Don Williams. Yet I felt this might work out best with so many conflicting interests. So far as I could tell he had no really close friends. (From then on it was obvious that Williams was very ambitious for something but I never found out exactly what for.)

On the 5th of November Williams sent out a sort of soothing letter to the entire staff. Shortly thereafter he announced ~~that~~ the appointment of Van Dersal as Assistant Administrator for Management. He had been personnel officer for awhile and then a regional assistant for management with Williams out of the Portland office. Van was bright enough but not eager to work. Salter had previously decided

on Carl Dorney who would have been a good one I think. Certainly he was a wonderful budget officer.

The reorganization did not come soon enough to relieve Dr.

W. S. Ligon from the great pressure against him stirred up by Glen (one-eyed) Fuller. He had a nervous collapse that put him in the hospital. It certainly was a shame that he had to take the treatment that he did. From then on I did everything I could to back him up and to protect him.

The Soil Science Society meetings were held that year in Dallas.

Hub Alloway and some of the others went but I staid in Washington because the state conservationists were called in to meet with

Don Williams. (During the whole of the autumn I spent a good deal of

my time preparing and checking statements about the nature, requirements, and use of soil surveys for different purposes besides extensive correspondence.

The schedule for disbanding the regional offices and changing over to the new organization was firmed up with them and there were many individual discussions about staff and the like,

At Hub's suggestion I wrote a long letter to Dr. Pierre about our situation including my conviction that I should stay on. If I left now it would be misinterpreted and make trouble. Further, I could see that a strong Soil Survey staff could have an important effect to improve the efficiency of the big expenditures under conservation operations. I saw the biggest difficulty ahead to be the maintenance of scientific standards. So long as the administrator's position remained Schedule C, it would be extremely difficult to attract a prominent soil scientist to it. I also emphasized the importance of having the soil scientists of the SCS within each state report to the state soil scientist. If the colleges really wanted to help they could emphasize the need for more soil correlation staff. (But I never did get any help from them on budget or otherwise. Never once did they appear before the congressional committees in support of the work!)

Dr. Pierre wrote to me nearly the same time about his conversations with Hub Allaway but he didn't have much luck talking him out of going

back to Beltsville. Bill was extremely worried about the future, but he was only one of a large number.

After all the wrestling with the personnel people, who should have been helping me, the GS-14 grades came through for the top staff of the Soil Survey.

Most of my time was now being taken up with the very complicated problems of working out staffing arrangements under the new organization and correspondence with my field staff. The biggest job was to find something for the regional soil scientists to do. This took a good deal of negotiation within the Service and with the state experiment stations. We decided to move Arnold Baur from state soil scientist to soil correlation. But New York absolutely refused to have anything to do with Henry Adams.

The big show in early December was the hearing before a sub-committee of the Committee on Government Operations on the reorganization of the Soil Conservation Service. The fact that Republicans had a slight majority made things easier for the administration. But it was bad enough as it was.

My mother was living with us and seemed to be feeling quite well.

Mary Alice had come down from Cambridge about the 13th of December and Joan and Robert were coming for Christmas. She was looking forward very much to this holiday and to meet Joan.

Just after dinner December 21 she had a sudden heart attack.

Apparently she never knew what happened; she just collapsed on the floor.

Lucille placed a towel on her head and called the physician who suggested ammonia fumes. She gave two short gasps but never recovered consciousness. The physician - Dr. David Clayman came in a few minutes and found no signs of life. He called the coroner who gave permission for us to carry her to the bed.

We called the undertaker here, and one in Ionia, Michigan; Robert in Cambridge; and old friends, the Saffords, near Palo.

Everything was so sudden and so shocking. By 11:00 p.m. all arrangements were made. The weather was so bad in Michigan that we decided to go by train.

December 22, Lucille and I went to the funeral parlor, chose a casket, and got our tickets. We had to get money because the total

Dale

cost would be well over \$1,000. I had also to make arrangements at my office and for the possibility of hauling furniture from Palo here.

The three of us took the B. and O. to Detroit at 5:50 p.m.

On December 23 we arrived in Detroit in the morning. It took some time to arrange our return reservations and to look after the switching of the casket to the Grand Trunk train to Ionia. Undertaker Dale Kaufman was on hand at Ionia. After dinner the Saffords came for us and we went out to their farm home.

On December 24 I sorted and listed Mother's papers that had been left in the safe with Ralph Safford.

We went to the Wingeiers for dinner (at noon) and then to the Methodist Church for the funeral at 1:30. A great many people were there despite the time on the day Before Christmas and the very cold and windy weather. It was bitter cold at the cemetery.

Franklin Austin took us to Mother's house and ~~xxx~~ we began the terrible job of sorting. In her desk I found her will, leaving one thousand dollars each to Robert and to Mary Alice with the rest, which was not much, to Lucille and me. Yet Mother left us a few shares of

Detroit Edison and Consumers Power stock that doubled in value during the next ten years.

We decided to sort out a few things that would be useful to ship home; to give away the remaining useful things, and ~~to~~ to destroy the personal things we didn't want and would be of no value to others. She did have one oil space heater that we sold to a stranger living in Palo.

My father had imprudently sold the farm at a relatively low price with the proviso that he and Mother would have use of their house so long as either lived. Thus the house now belonged to a Mr. Way. He called and said that we need ~~be~~ in no hurry.

Fortunately the tenant had started some fires in the house but it was still very col^d. We worked that evening until supper time, ate at the Wingeiers and went to the Saffords for bed very tired indeed.

December 25 the three of us went back to the old Kellogg farmhouse to sort and to burn. We returned to the Saffords for Christmas dinner at noon and they came back with us and took a good deal of the furniture. We gave things to other neighbors and to the tenant.

December 26 we went to Ionia and consulted Mother's lawyer, a Mr. H. H. Gemeund. We gave him papers as follows: (1) a copy of her will, (2) the death certificate, (3) a receipt for the filed will, and (4) a receipt for the Ionia funeral expenses. We then went to the bank and took out her savings of \$502 and I wrote a check for \$465.51 for the Hyattsville funeral expenses. At the bank we took \$40 in cash and a check for \$660.13 which covered her savings and checking accounts. We went to the County Library and offered them the books and a man came later for them.

We went back to the Wingeiers for lunch and then to the old farm^chouse for more sorting. Many of the things were given to the Wingeiers for them and their daughter.

We spent all day Sunday, December 27, at the old house and placed all of the things to be picked up by the truck for Hyattsville in the parlor except for the refrigerator in the kitchen. We saved the fine old furniture that had belonged to my Kellogg grandparents. The nicest piece was an old walnut secretary of about 1830. Besides were chairs, including a platform rocker, a bedroom set, two tables, and the nice old books my grandmother had.

On Monday, December 23 we visited the lawyer in Ionia and completed the papers to close out Mother's estate. After lunch we went to the probate court and everything was filed and settled.

I was told that I must send forms and notarized receipts for funeral expenses.

On December 29 we were up early and took the train for Detroit. Mary Alice had gone two days earlier.

We reached home December 30th and took care of all of the remaining items on my mother's estate at our bank.

This turned out to be a very difficult experience partly because of the suddenness of her passing and partly because it broke our remaining tie with the old home. Perhaps worst of all, was the sorting out of all the things she had cherished and cared for so many years.

Robert and Joan arrived from Boston Thursday the 31st and stayed until Sunday. They all left for Cambridge on Sunday January 3rd.

1954. When I returned to the office Monday, January 3 I found that my staff had been working well and had sent forward to Mr. Williams several tentative new locations for correlators and principal soil correlators.

While I was away Ray Roberts had written me from Berkeley saying that he really wasn't qualified for this enlarged responsibility. Ray had done that before and I knew he didn't expect me to believe him, but the staff all did believe it. So I called Bill Johnson in Lincoln, explained the situation and asked him if he wanted to go to Berkeley to head the western work. He agreed. So then I got Aandahl on the telephone and he agreed to take over the Great Plains.

It turned out that Bill Johnson had the foresight also to write to Ray Roberts and had a reply from him confirming that this was his desire. And Bill had a copy of Ray's letter to me. When Bill got out to Berkeley the air was very chilly. Word had got around that Bill had run Ray out of his job. In a fit of pique one day John Barnes, state conservationist in California, told Bill as much. Then Bill showed him the whole file. Ultimately this put an end to the rumors.

The Service had also got out a general reorganization memo with target dates for closing out the old regional offices.

We had difficulties placing people from the regional offices and getting agreement with the experiment stations on some of the state soil scientists. We got two very good men into the correlation work from the old SCS - Arnold Baur and Dr. T. B. Hutchings. Paschall moved into Nygard's old job. Marvin Lawson came in but did poorly. I was unhappy that Carl Ferguson quit and went back to ECA. Fortunately Simpson, former state soil scientist in Texas, left. Since about 1945, he had learned nothing nor forgotten anything. (But after that he went with the Indian Service and quite effectively blocked reasonable cooperation for many years.) Several state soil scientists were finally accepted by me grudgingly and about three others were confirmed without my approval. Of this group of 20 or so who seemed doubtful to me, two turned out reasonably well in time, a few retired, and the others dragged their feet for many years.

By this time at least some training programs had been under way in each state. But the big job was ahead. Perhaps one-half of even

the state soil scientists had to start with basic training on how to describe a soil. Roy Simonson was especially pessimistic but I was confident that there were some real good men in the old SOS if we could just spot them and get them started. This turned out to be true. Yet we had trouble because the personnel division was unconcerned with either scholarship or ability and it was hard for me to bump the nearly dead ones that had been "loyal to the Service" even though they had no other qualifications. All of this was a tiresome dirty business but it had to be worked on. Had I been willing to be a "good fellow" and recognize the old "loyal timekeepers" we should have had a horrible staff and I would have lost more of the good men than I did.

But besides organization many other things had to be done. It seemed important that I go to the Fifth Congress of the International Society of Soil Science being held in Leopoldville, Belgian Congo, during August. This arrangement had been firmed up during the previous November. Also I accepted an invitation to visit the Gold Coast on the way to the Congo and the British Advisory Service on the way ^{back} back. I decided to accept Jurion's invitation to make a full-dress

speech on Soil Conservation at the Congress in order to reverse the wrong ideas that many of these people had had from reading Bennett and Lowdermilk.

We also started to prepare a detailed administrator's memorandum on the conduct of the Soil Survey work. This had to be policy, direction, and some plain schooling. We worked very hard at it but it took a lot of time to get it written with full staff agreement.

During these months I also wrote a good many other statements including a rather detailed one that I discussed at great length with the field representatives and other top staff.

As I went over it with them I left nothing much to the imagination and covered the following outline in detail:

I. General outline of the program.

1. The consolidation of the former Soil Conservation surveys with the National Cooperative Soil Survey. Reasons and firm intentions.
2. The two jobs of the Soil Survey, (1) to guide technical assistance, and (2) to supply the classification and maps

needed for research synthesis and to meet the needs for soil maps of other state, federal, and local agencies as well as private persons.

3. The equal and vital importance of sound science and quantitative interpretations for us.
4. Staff organization including the detailed organization and functions of the staff in Washington, the Washington-field staff, and the state staffs.
5. Policies of cooperation to achieve a sound basic soil survey.

II. Major problems.

1. Serious training needs of Service and scientists.
2. Need for many more men skilled in soil correlation.
3. Need for movement of ^{men} for training.
4. Importance of nation-wide system of classification and interpretation.
5. Great untapped opportunities for fuller use of soil surveys within the Service.
6. Importance of further state cooperation.
7. Importance of prompt publication and costs involved.

8. Scheduling and management of cartographic services.

III. How the field representatives can help.

1. Explaining to the state conservationists their new responsibilities and the importance of the basic soil survey to other activities.
2. Help us find both the "worst" and the "best" places.
3. Help insure the appraisal of the soil survey needs of other agencies.
4. Some of our immediate problems stem from incompetent people in technical positions.

At meetings of state conservationists I had to go over the details of all of these points again and again in the years that followed.

By the end of January we had most of the major shifts among key people fairly well settled.

Early in February, Director Walker of the Utah Station, who was chairman of the Land-Grant Soil Survey Committee, gave me confidentially copies of most of the letters he had received from the state experiment stations. This was quite a mixed bag of ideas but on the whole helpful.

Two that were highly impracticable then were put forward, (1) to transfer the whole thing back to ARS, and (2) to have soil correlation decisions made by state committees.

All the principal soil correlators, but especially Bill Johnson, sent me frequent appraisals of the work, of the ^eman, and of the gossip. This large volume of personal correspondence from the principal soil correlators helped me no end and gave me many opportunities to help them. But it took a great deal of time.

Finally at the end of February Dr. Hub Alloway returned to Beltsville. Despite the progress we were making he never got over his trip to the Southern state offices. So then we had the job of finding a successor. I spent a lot of time on this. Aandahl would have been the best man but I couldn't replace him so I settled on A. A. Klingebiel. He reported for duty near the end of April and I had a serious training problem for more than two years. He was conscientious and worked hard but his logic and writing were inadequate. We both sweat together and finally he came through.

March 9 I spent in New York giving a lecture on Our Garden Soils to the Men's Garden Club of New York City. Most of these men were very highly placed. I went out to their show and saw the president of one of the biggest banks on Wall Street down on his knees showing a Bronx housewife how to make chrysanthemum cuttings. I am sure she would have died of fright had she ^{he} known whom he was. These important men took turns at this sort of thing for two or three days. I don't know where else in the world one could see something like this.

Then, of course, I had my own garden to worry about.

Ever since the spring of 1943 after moving to 4100 Nicholson Street in Hyattsville, gardening had taken up a good deal of my time. Actually, it didn't really compete with other things much because I worked out as many problems in my mind in the garden as I ever did in the office. And the exercise helped some to relieve tensions. The soil at our place was very poor for gardening. It was too clayey, too sloping, and had a hardpan from a previous cycle of genesis.

When we started a victory garden during the war, I told Lucille that this was quite a gamble. "If we have a good garden people will

say, 'he should have; he is a soil scientist.' But if we have a poor garden they will say, 'Look at that garden! And he calls himself a soil scientist.' "

Mr. Allin^(u)son who was business manager of the Bureau of Plant Industry for many years, helped me get started with azaleas. Then after the war we rather rapidly dropped all the fruits and vegetables and kept up an azalea garden with a few other things. By 1960 over 35 tons of stone had been set in it so that most plants had their individual stone terraces. But as the plants grew we learned which ones to favor and which ones to get rid of. Also the little terraces had to be continually adjusted. This garden was planned as an outdoor living room and we used it a great deal in the summer, especially before air conditioning.

Every year we had a big bustle in the spring to get started and every autumn the job of gathering up the leaves and making compost, to be turned the next June, and used in March or April after that.

finish

During March I made the first draft ^{of} for a paper for an International Population Conference in Rome. FAO asked me to deal with the subject, How shall we discover our agricultural resources? Because of the commitments already made it wasn't possible for me to attend the meeting but the paper was included with the transactions.

The Work-Planning Conference of the National Cooperative Soil Survey was held in St. Louis March 15-20.

Insert pages 594⁵a, 594⁵b, 594⁵c, 594⁵d, and 594⁵e.

MARCH 15-20, 1954

This conference had a very heavy workload. Mr. Williams came for awhile and saw how it was handled. We had nearly 30 people, which was most too much but ^{remained throughout} not all of them stayed. Besides three of our Canadian associates we had with us Professor L. J. H. Teakle, of the University of Queensland, Australia.

In my opening statement ~~I again~~ re-emphasized the importance of both the scientific work and the interpretation work. We were developing and improving our system of classification ^{but} and we needed a sound and consistent theory of soil survey interpretations.

The proceedings of this conference would make a small book. Our first emphasis was given to the new system of classification. This was a staff undertaking begun in 1951 under the chairmanship of Dr. Guy D. Smith. During our first staff conference in 1951 he submitted a brief outline that we agreed to call "the" First Approximation." Then in April 1952 he developed a somewhat longer outline as a "Second Approximation." This had been revised during the following year and in September 1953 came out as a "Third Approximation." This draft formed the basis of the discussion at this meeting in St. Louis. The discussions varied all the way from basic criticisms to trivia; but on the whole, it represented considerable progress.

On the other most important subjects the conference was organized into committees for two days and these committees reported during the following two days. The subjects discussed including the following:

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1. The recent improvements in guidelines for published soil maps and maps with special emphasis on the detailed soil maps that

included all the boundaries significant to soil use.

2. Mr. Orsini led a useful discussion on the opportunities with current cartographic procedures.

3. We had a useful discussion on improved sampling for soils to be examined in the laboratory.

4. We spent sometime in discussing what ^{was} we expected in the way of annual plans of soil survey operations from ^{each} the States.

5. The conference took up the problems of soil classification and mapping in extensive grassland and forest areas. People were there from other technical areas of the Service ^{to help out. It soon became clear that} but a lot more work had to be done in the field by both soil scientists and plant scientists ^{in order} to develop ^{for} more quantitative procedures for interpreted ^{any} soil surveys ^{in terms of range grasses and forest trees.}

6. Two committees had worked on the new system of classification. One of these dealt with what we called at that time "low families", ^{which} that were the lowest grouping ^{of} the soil series in the system, and "high families", which were groupings that could come between the great soil groups and the low families. ^A The big problem ^{than} here was to consider principles for intergrades among the groups. The low family problem involved judicious selection of the most useful and most critical characteristics from among a great many.

7. In 1953 the conference had developed a start toward the classification of organic soils. At this conference ^{we} they went a bit further ^{but} and it became obvious that Dr. J. E. Dawson would need to spend a good ^{suggest} more time in the field in order to ~~affect~~ a useful system.

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8. A committee wrestled with the problem of erosion classes to be used in soil classification and mapping. In the old SCS surveys erosion mapping had been carried almost to the point of absurdity and some way had to be developed to bring order into ^{the system to} it and relate the erosion classes to soil series and types. ^{it was} explained that we were not mapping erosion and could not. The hazard of erosion was a matter inherent in the nature of the soil type, whether eroded or not. We needed units for mapping eroded soils. Some soils were strongly eroded even though the hazard was low; whereas others were little eroded even though the hazard was high. These ideas were so contrary to the old ^{ideas} practice in the SCS that it ^{was bound to take} took a little while for people to make the essential shift in concepts.

9. Dr. Whiteside of Michigan asked to head a new committee on soil horizons. This was arranged for and his committee gave a report. We had some quite warm discussions and some improvement in understanding but without any improvement over the system in the Manual, nor did one come for several years.

10. A continuing committee on criteria for soil series, type, and phases reported, especially ^{along two} in line with requirements ~~that were needed to~~ ⁽¹⁾ make possible orderly groupings of soil series into an improved system of soil classification and ^{(2) planations for} to provide proper names for eroded soils and other phases. ^{planned}

11. ^{Early} As soon as we recognized the enormous training problem before us, especially for the soil scientists who had grown up in the SCS, This committee was extremely important and had a long report. We explored the opportunities for graduate study and for the detail of soil scientists among States. The discussion was useful. In order to emphasize the

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great problem I had to point out the need for general education as well as ~~the~~ ^{for} technical training. Our best immediate tool turned out to be ~~the~~ correlation training schools that were operated for several years by each of the principal soil correlators. Much later, we were able to establish a special institute operated by Cornell University.

12. We had set up a new committee on the problem of making more effective use of the information in soil surveys. In the former surveys in the SCS the whole question of yields had been side-stepped. Yield estimates were essential for getting going with agricultural interpretations that people could use. Not until this was done ~~were~~ ^{could} people ^{be} able to make ^{if any} much sense out of the old SCS "Land-use capability classification." I knew this would take a long time because it was one of Bennett's most sacred ^{of} "Sacred Cows", and of rather limited use. Yet this came about faster than I expected, ~~because~~ ^{most} of the State conservationists really didn't think much of it but had never been able to say so. The rest of this discussion was a good educational document.

13. We also had a committee on conventional mapping symbols. This matter had not been controlled well in the old SCS surveys. In fact, it had ^{also} gotten a bit out of hand in some of the ^{basic} ~~old~~ soil surveys. Here too, matters of precedent had to be blasted out and it took a few years to do it.

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14. Dr. T. B. Hutchings led an interesting discussion on available moisture in soils. We opened up the problem, but this too turned out to be a difficult one.

15. We had a brief discussion of soil drainage. Both before the conference and for years later certain individuals complained about the drainage classes in the Manual yet no one was able to come up with anything so good.

16. We had submitted ^{to the men} a copy of the draft of a memorandum on soil surveys and got quite a few suggestions for its improvement, largely because some of the readers of it had misunderstood it.

Finally we wound up the conference on Saturday, March 20 with highlight emphasis on the main jobs ahead. Despite the old differences of view most of the men took a good attitude of putting all that behind in trying to find the best way to go ahead with our new job.

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I spent April 6 and 7 at the state SCS office in Ohio. The survey work was going badly in Ohio because of the weakness of our state conservationist, Kennard, the ineptness of the University, and the empire building of the State Department of Natural Resources. It was difficult to keep Kennard and Morse, the state soil scientist, on the subject. They wanted to know all the latest gossip in Washington. We discussed the urgent need of an assistant state soil scientist for soil correlation. Kennard agreed and wanted to find a way to use it for bargaining with the University!

In the afternoon of the 6th we had a rambling unproductive conference with Kennard and Morse of the SCS, Ferguson of the Natural Resources Department, and Garth Volk and Hollowaychuck of the University.

During the forenoon of the 7th I went over the plans carefully with Morse. He had too many other jobs to do and no direction on any of them. Because of the unresolved relationships, management for planning soil surveys, for soil correlation, interpretation, and reports was poor. We tried to figure out how to get some help on the details in the field from Frank Carlisle later. The State Department of

Natural Resources had 14 men in the field plus three others and the Service had 19 men in the field plus ^{two}~~three~~ others. The University gave only token help but hoped to increase the laboratory work.

Both Kennard and Morse were over their heads and I realized that they must have a great deal of help from the Washington-field staff to get anything done well.

In addition to work on the basic memo about soil surveys and correspondence about April 27 I gave a luncheon address at the United States Chamber of Commerce as a simplified statement of food potentials to counteract the historical approach then current among economists.

The Chamber circulated this as, We can have plenty to eat.

In May we finally completed the basic memorandum then called SCS memorandum No. 47, which later was reissued as Soils memorandum No. 1. It came out a few weeks later dated June 10.

With some misgivings, I agreed to let Simonson go to Brazil to help the men in FAO get started with the first really serious soil classification work. Since Portuguese was his native language, Bramao was very keen on this work and it seemed to get off to a

good start. Later we discovered, however, that Bramao's lack of interest in agriculture carried over to the Brazilians and the work didn't contribute nearly so much to solutions of the severe problems of agricultural development as it could have.

At the end of May I spent a couple of days at Ithaca, New York going over the program of the Plant, Soil, and Nutrition Laboratory and some time with the people in soils at the University. While at the Laboratory, I agreed to try to assign Dr. Kubota there, on our payroll, to help with the research and to maintain liaison between our research and that of the Laboratory.

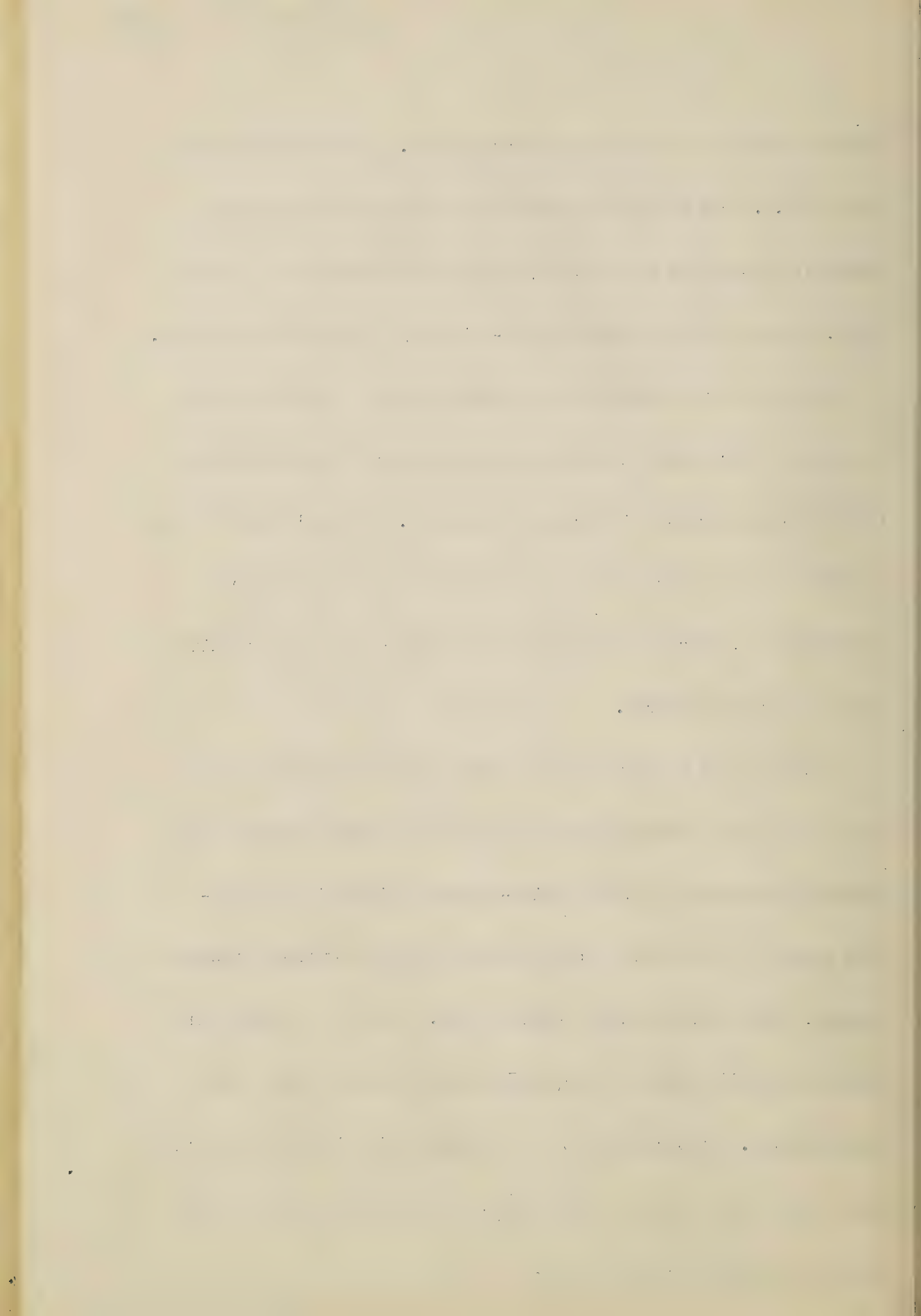
In the middle of June, Lucille and I drove to Boston for Mary Alice' graduation at Radcliffe. I had brief visits with Dr. Black and Dr. Galbraith, bought some books for myself and for Robert, and attended the Harvard Commencement. This ^{was} ~~is~~ a very colorful affair.

The academic procession ^{was} ~~is~~ led by the sheriff of the county. The students compete eagerly for the privelege of giving the English oration and the Latin oration. At the appropriate moment, the President asks ^{ed} ~~s~~ the candidates for the A.B. degree to draw near, and he

welcomed them into the company of educated men. Later the candidates for the Ph.D. drew near and he welcomed them into the company of scholars. Robert had a way to go yet before he "draws near" for his Ph. D. Then he had two hurdles left - his army service and his thesis.

Among the many documents that I prepared was a considerable one as a part of our budget justification on the role of the Soil Survey in the economic growth of American agriculture. We didn't have a chance to defend the Soil Survey and get support for it on the basis of "conservation", which had been formerly conceived in the SCS as little more than erosion control.

About June 21 I had an urgent letter from Dick Braefield saying that he was having trouble getting security clearance because of his supposed relationships in the Soviet-American Friendship Society - the same point that caused me a lot of trouble and the government needless expense. Fortunately with my complete file, I was able to send him a statement, with dates, of all communications where he might have been involved. It turned out that this helped him find some letters that he had been unable to find before. With my statement and these letters the matter was cleared up.



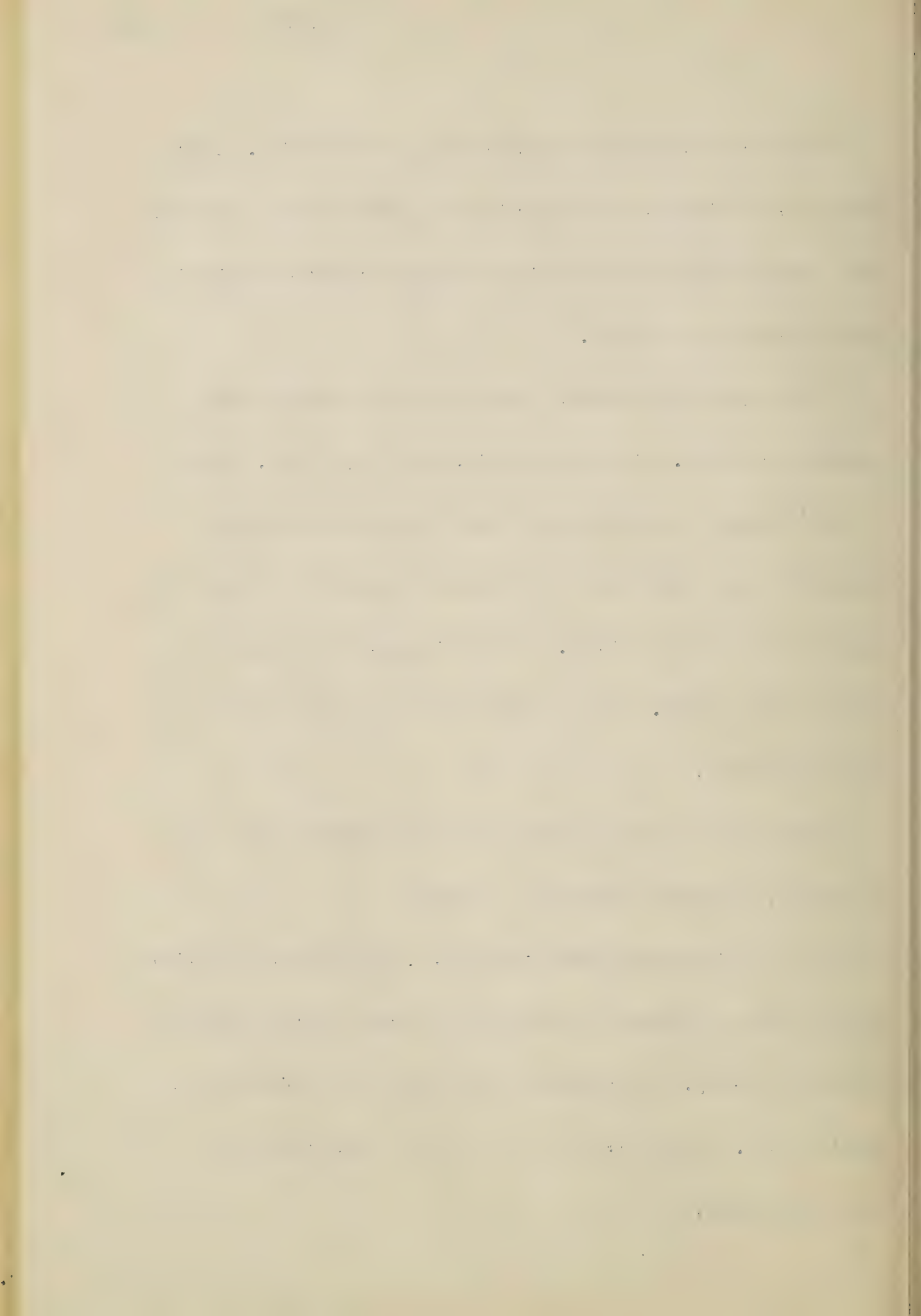
I spent July 12 and 13 in Madison with our state office. I met with the soil scientists of the Service and talked with them about the work. Then we ^{went} ~~went~~ out into the field where they received training in making descriptions of soils.

I had dinner chez Schwers. When he came in the house he was carrying a suit box. His wife asked him, "What do you have, Marvin?"

He replied, "I saw a suit in a store window that I liked so I bought it today." She told him to go try it on. In a few minutes he came down with the new suit on. His wife inspected it closely without saying a word. Pretty soon he went back upstairs and put on his other clothes.

Months later I asked him whether he got to keep that suit. As I expected, he replied softly; "No, I didn't."

I got to like Marve Schwers very much. At first he was suspicious of me but we got to know one another and he became a strong supporter of the Soil Survey. He pretended to be tough and hard boiled but he wasn't at all. He was one of the few men who I think really died early of overwork.

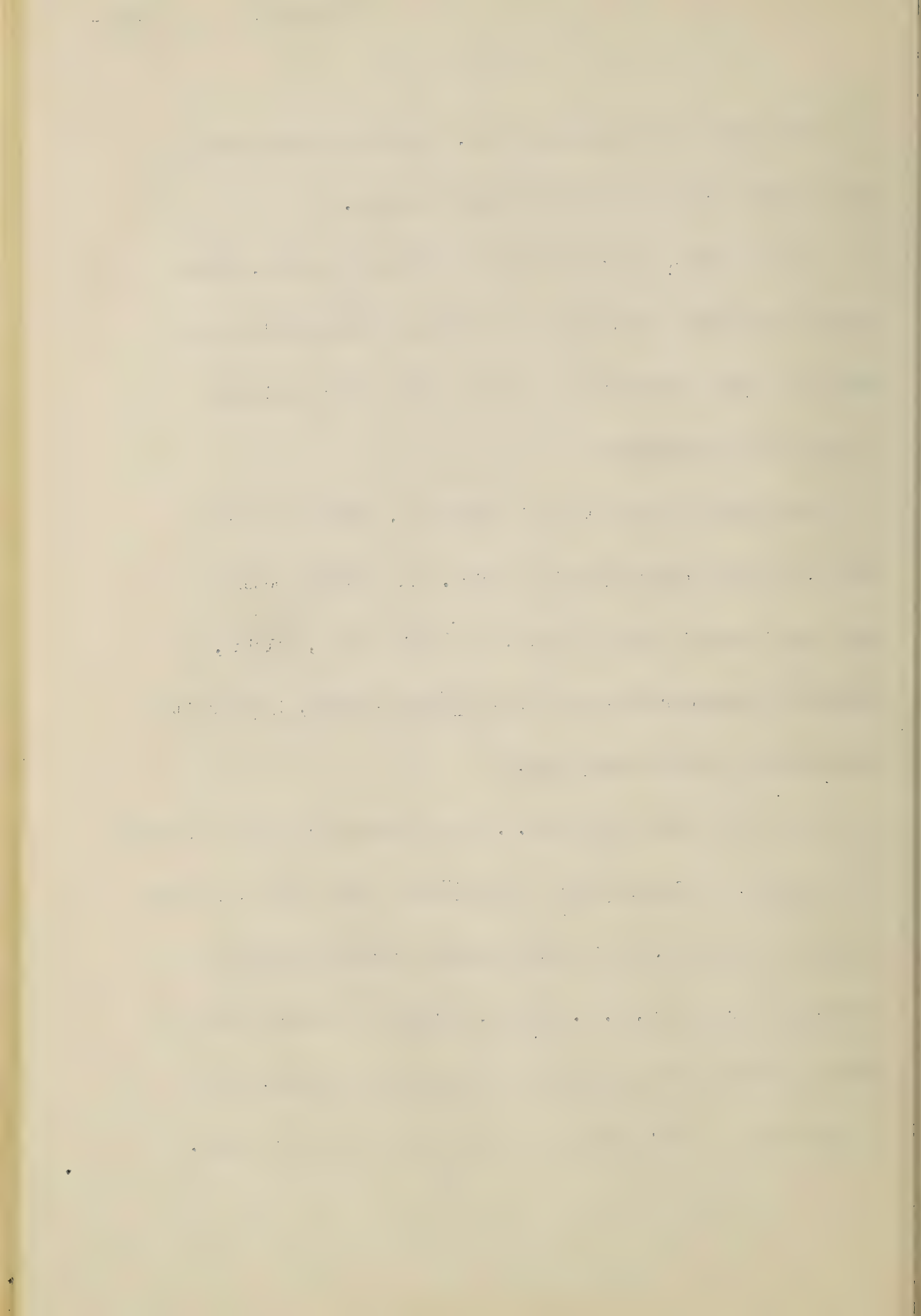


Arrangements were completed for Dr. Kubota to be transferred to the Laboratory at Ithaca on the first of August.

Also in August, after I had left the office for Africa, Harvey Oakes returned from about two years in Turkey where ^{he} had helped them get a soil survey going and had prepared a good general soil map of Turkey with descriptions.

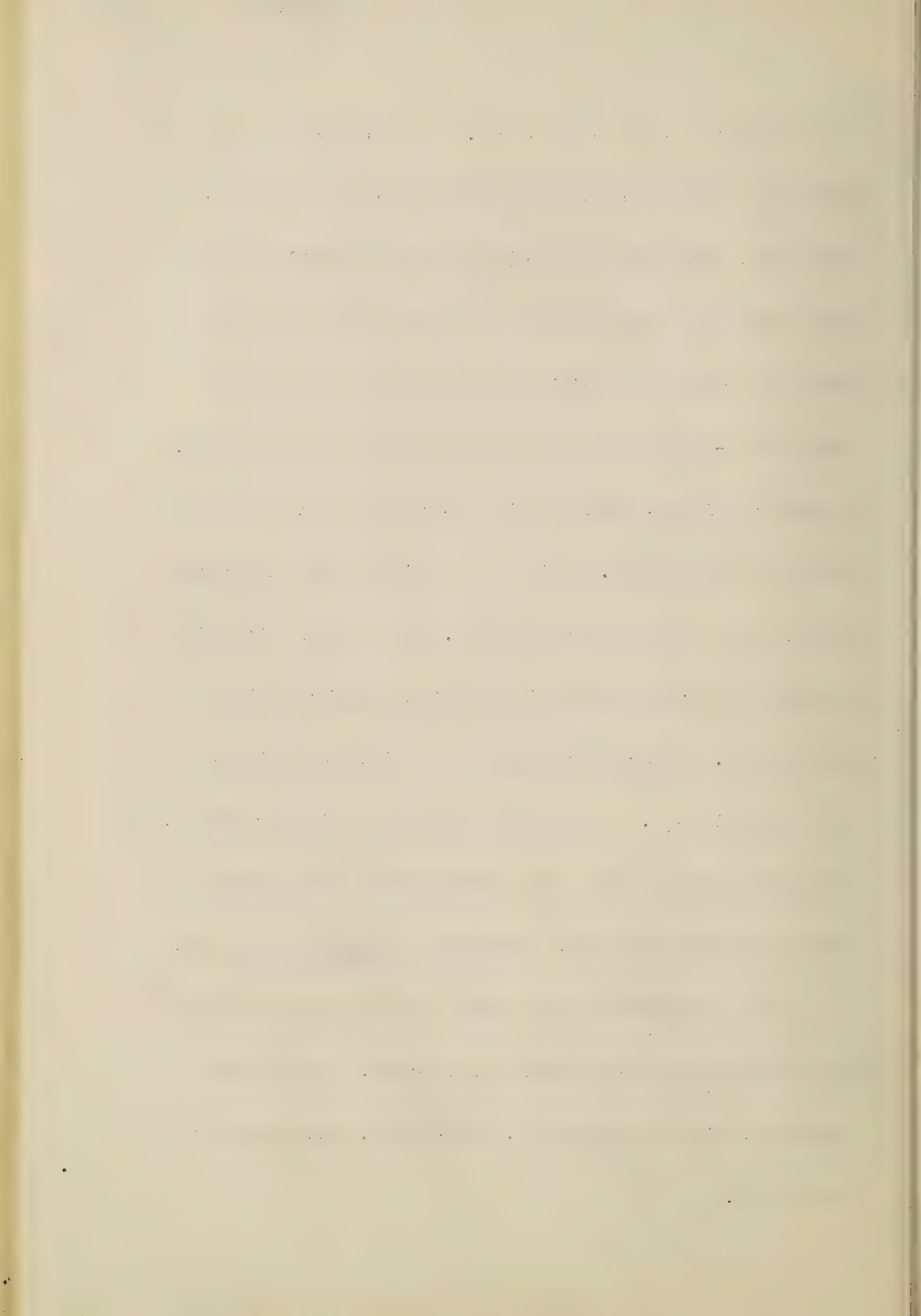
The morning of August 1 I left Washington, sweltering at over 100° F., for New York on my way to Africa. The detailed notes for this long journey in the Gold Coast, The Belgian Congo, Belgium, and the United Kingdom are set forth in the African Journal, 1954 and its considerable appendix volume, (q.v.)

We left New York about 5:00 p.m. and were down for an early breakfast in the Azores. I got to Accra in the Gold Coast about 3:45 a.m. Here I was met by Tom Mead, who was then Permanent Secretary for the Ministry of Agriculture; C. F. Charter, Director of the Soil and Land-Use Survey; and H. Brammer as chief scientific officer. I got a little sleep in Mead's house and after lunch went into the field.



I enjoyed this work a great deal. I was able to see cacao production and had a good look at the black soils (Vertisols) of the Accra Plain, which were to be irrigated under the proposed Volta River Scheme. For a part of the time I staid in the old Danish castle as a guest of the British Governor General, Sir Charles Arden Clarke - one of the two wisest men I had ever met in Africa. (The other is Jurion) Every evening after dinner he lectured me on Africa for about an hour. During the uprising he had put Nkrumah in jail and now they were great friends. The Governor still held the power and Nkrumah and his ministers were really "practicing" at government. Tom Mead was teaching the Minister of Agriculture how to be a minister. I had two long conferences with Nkrumah and, at his request, wrote a report with recommendations based upon my observations and conferences. (Included in African Journal, 1954.)

I also had interesting conferences with Commander Jackson who was the chief planner of the Volta River Scheme, and one short teatime with his more famous wife, Lady Jackson, better known as Barbara Ward.



Near the end of the visit I became convinced that I had not been invited primarily to look at the soils on the Accra Plain. C. F. Charter was a strong and unusual individual. He hated administrators. He had been fighting them most of his life to protect the scientific integrity of his work. He had managed to insult the Governor's Lady and others. I had known both Tom Mead and Professor Phillips of the University of Accra. Apparently someone high in the government had raised the question of whether Charter was really "on the rails". Phillips and Mead knew that I would be going to the Congo and suggested the stop in the Gold Coast. Since I had no axe to grind in that country and hadn't met Charter personally before, I think they hoped to get an unbiased opinion. I had to tell them that despite his peculiarities and some differences between us I thought they were very fortunate to have him because he probably had the best soil survey program in Africa.

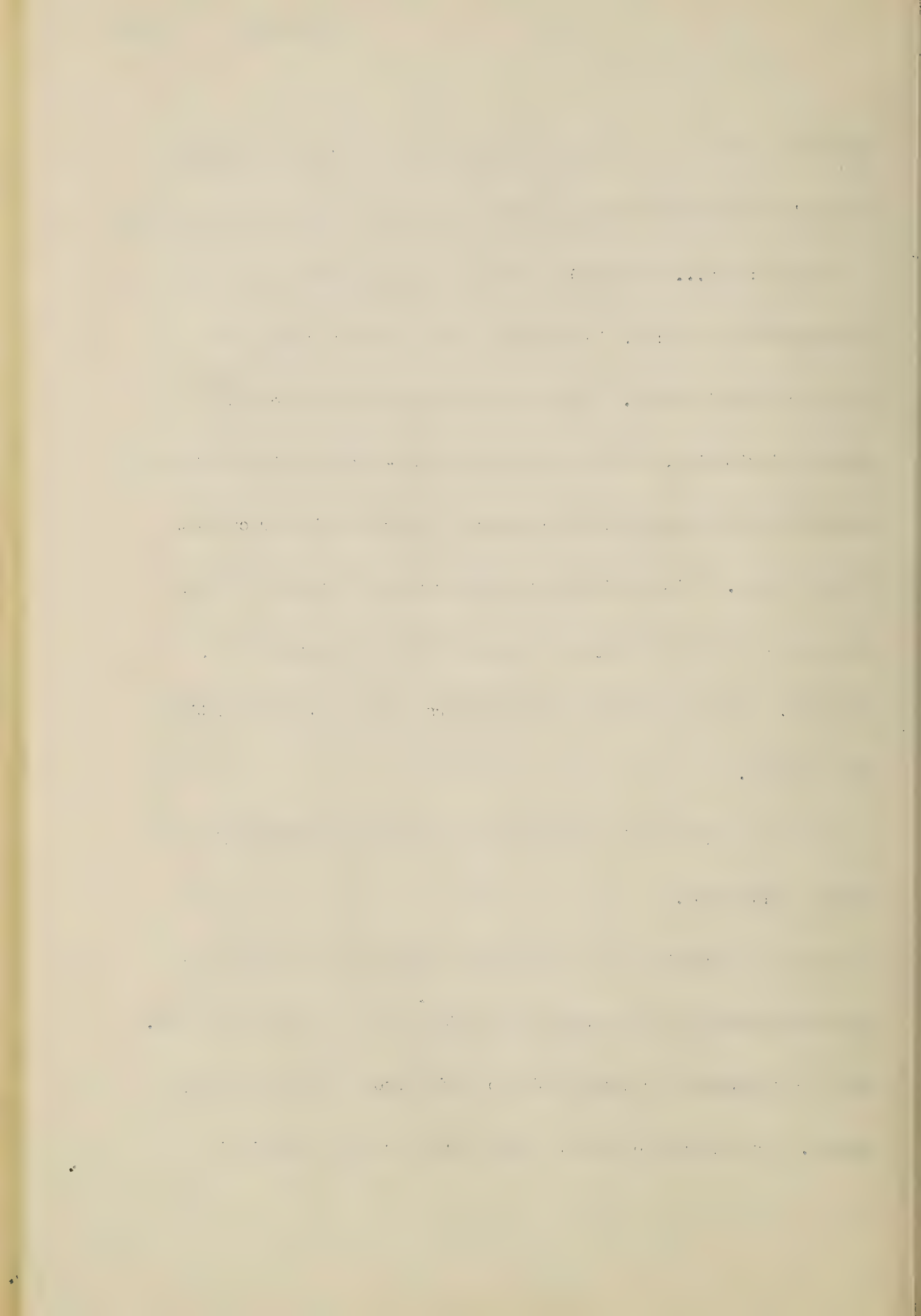
Charter was arrogant ^{to} ~~at~~ his own staff but they knew him and loved him. (In the African Journal, 1954 I tried to capture some of his office manners, pages 21 to 24. Later when he visited me at home I

showed him this part of the Journal and he just chuckled. Basically he didn't take himself quite so seriously as most people thought he did.)

About 1:30 a.m. August 10 I left for the air station and arrived in Leopoldville at 8:35. It was very good to see Leo again and Director General Jurion. I had gone early to Leopoldville, at Jurion's invitation, to take part in the Inter-African Soils Conference sponsored by the Committee for Technical Cooperation in Africa South of the Sahara. This Committee was sponsored by governments and men could go to that officially and then stay for the Congress that followed. Thus many of the kinds of papers given at the two meetings were similar.

Gradually during this Conference my old friends appeared from the various continents.

We had a reception at the City Hall of Leopoldville at noon and a business meeting of the Society the afternoon of the 15th of August. Here I got support for the election of Emil Truog to be an honorary member. The Congress opened the next morning and the technical



sessions began in the afternoon.

The Governor General of the Congo gave us a nice garden party the afternoon of the 16th at which I was able to persuade Professor Von Stralen to send Professor Tavernier to us in the United States again.

I gave my general lecture on Soil conservation at the first plenary session on August 17. I had worked very hard on this paper to redefine soil conservation positively and to emphasize the constructive opportunities for production. This was a real lively session, partly because of the extreme contrast with what Bennett, Lowdermilk, and the like had said. The discussion period lasted from 10:40 a.m. to 4:00 p.m. with a short break for lunch. I was about worn out at the end of it. But it was worth it.

We had many interesting papers and debates. Some were very good, a few were silly. The night that Edelman and some of his associates arrived we had a battle royal over a paper on reconnaissance soil surveys with aerial photographs by Buringh. I told them that if he presented the paper like he had it in the draft I would tear it to

shreds on the floor. He apparently took this seriously because he gave a far better paper than the one circulated in advance. (This point of view, made the other Dutch papers on this subject in the years ahead far more reasonable than they had been.)

At the end an agreement was made to hold the next Congress in Paris in 1956. Unhappily, Dr. Robert Ruhe who had gotten a special fellowship to come to the meeting, had an attack of ulcers. With the help of our Consul we got him a place on the plane and sent him home.

On the 22nd of August we had a nice holiday with a boat ride on the Congo River and a brief visit to Brazzaville, where I took a chance and got some perfume for my women folks.

On August 23 we took the train at 6:00 a.m. for visits to the Low Congo. The exhibits there had been very well arranged, especially a long, deep profile in the Red Latosol north of Gimbi. This trench went from the crest of one hill, across the valley, and to the crest of the next hill. It was the most dramatic pedological extravaganza I had ever seen. It took 50 natives ten days to prepare it.

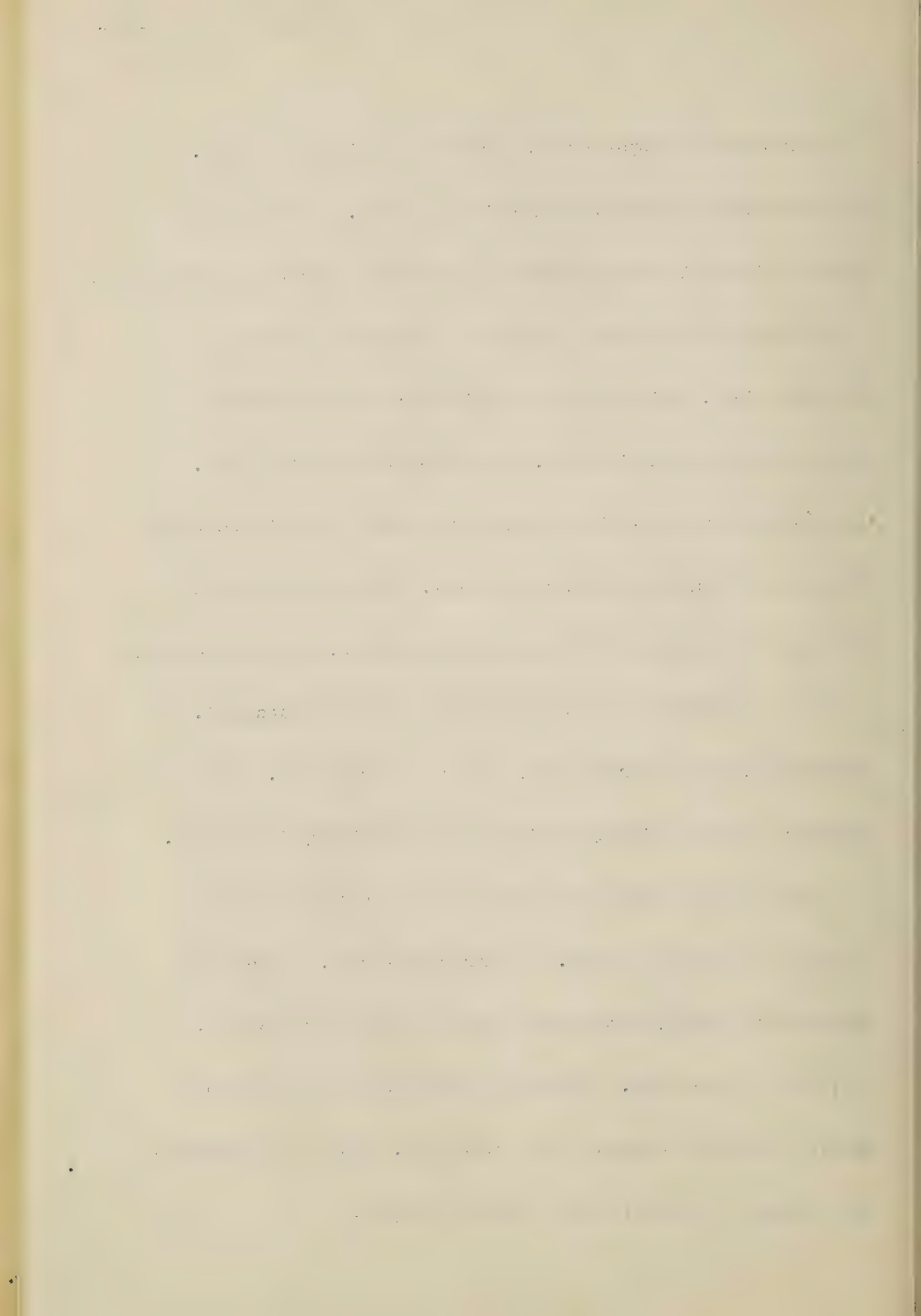
We returned to Leopoldville by train the evening of the 25th.

The next morning we went to Stanleyville by plane. In the evening we had a little sightseeing especially at Stanley Falls on the Congo River.

On August 27th we took a riverboat to Yangambi on the banks of the Congo River. This was a lovely trip near the dense tropical rain forest along the big river. The temperature was just right. In fact during all my visit to Africa that summer I experienced no heat ⁱⁿ anything like Washington on the day I left. The natives came out with their canoes and gave us an uproarious welcome, mainly for Jurion.

We had an excellent go at the soils and research at Yangambi. Everything had been extremely well prepared and organized. The corridor system of shifting cultivation was beautifully illustrated.

During our stay there the natives put on a boat race followed by some very emotional dances. I remarked to Jurion, "Suppose all this emotional energy were directed toward revolution?" He said, "Oh, that is impossible." (When men as wise as Jurion can be so wrong, what can be expected of the rest of us. [?] But if the government had listened to Jurion it would not have happened.)



On August 31 the group took the boat back to Stanleyville on the placid, lazy, old Congo under a bright and lovely sky. We had dinner that evening in the one nice place in Stanleyville, the "Pourquoi Pas."

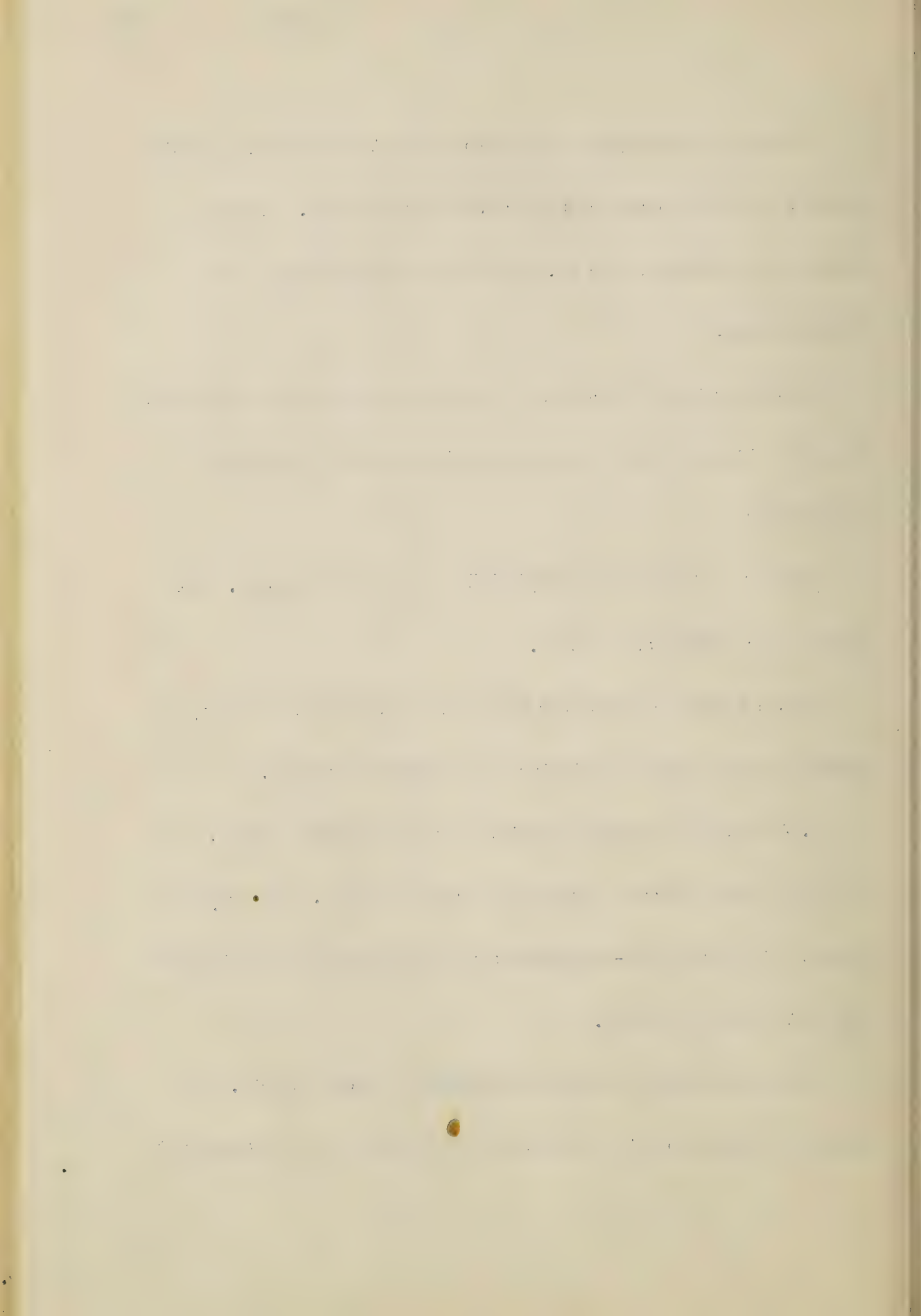
In the morning of September 1 we went by plane to Elisabethville. Here our hosts also had good demonstrations and some interesting field trips.

We took a train over to Jadotville and saw the smelters. But most of our effort was on soils.

On the evening of September 4 came the final dinner with many pretty speeches and funny stories that ended the Congress.

M. Jurion had arranged a special tour of the Lufira Valley. The INEAC was almost forced to start some research there. The area was covered with heavy tree-less savanna and the botanist could visualize all this changed to wheat!

Tavernier, Edelman, Stephens, LeBrun and I made the trip. We flew to a little town in the plateau and were met by Van Oosten with



a station wagon and driver. He was not too alert and we were late to our little hotel just near the beautiful Kiubo Falls. Here we found four more Belgian soil scientists and we explored the valley by car and boat. The Falls were wonderful to look at but I found that evening very noisy to sleep by.

This valley was probably an old lake in which the river now flowed with natural levees along its banks. The soils were mainly Tropical Black Clays (Vertisol) with gilgae and patches of interior Solonchak and weak Solonetz.

When we returned to Elisabethville, Edelman and I argued against any development without major works for flood control. Jurion explained that they were committed to do something in the way of a station so I advised him to make it as little as possible. Much nearer transport were good Red Latosols that needed ⁽ⁿ⁾mailly heavy phosphate fertilization.

I went by plane from Elisabethville to Leopoldville along with Brammer and Charter on September 7. The next day I had some interesting discussions at the American Consulate and left for Europe at 6:30 p.m.

This was my first experience with a sleeper and it certainly was helpful except for that contemptible thing called a "continental breakfast." served at 7:15 a.m. the next morning. But we were down in Geneva at about 9:00 in the morning (September 9) and had a proper breakfast. I had been able to get things for my lady folks and here I got Robert an Omega watch.

I reached Brussels just before lunch and was met by people from INEAC. I was told that I would have dinner chez Mme. Jurion that evening and in the afternoon they were prepared to take me to Antwerp to an art exhibit. But I couldn't take it.

I went to Mme. Jurion's and had to leave by 11:00 p.m.

On September 10 Guy Smith was also at the hotel and Tavernier came directly from the plane in a little car. We all went to his house in Ghent. How we ever managed the baggage I'll never know. Dr. Smith was in western Europe for the first meeting of the European working party on soil classification sponsored by FAO and chaired by Tavernier. Since each country had a different system of notation they all agreed to follow the Soil Survey Manual to have common soil descriptions.

In the evening of September 12 I took the plane to London. The next day I went out to Rothamsted. September 14 I spent in London calling on old friends and doing a little shopping.

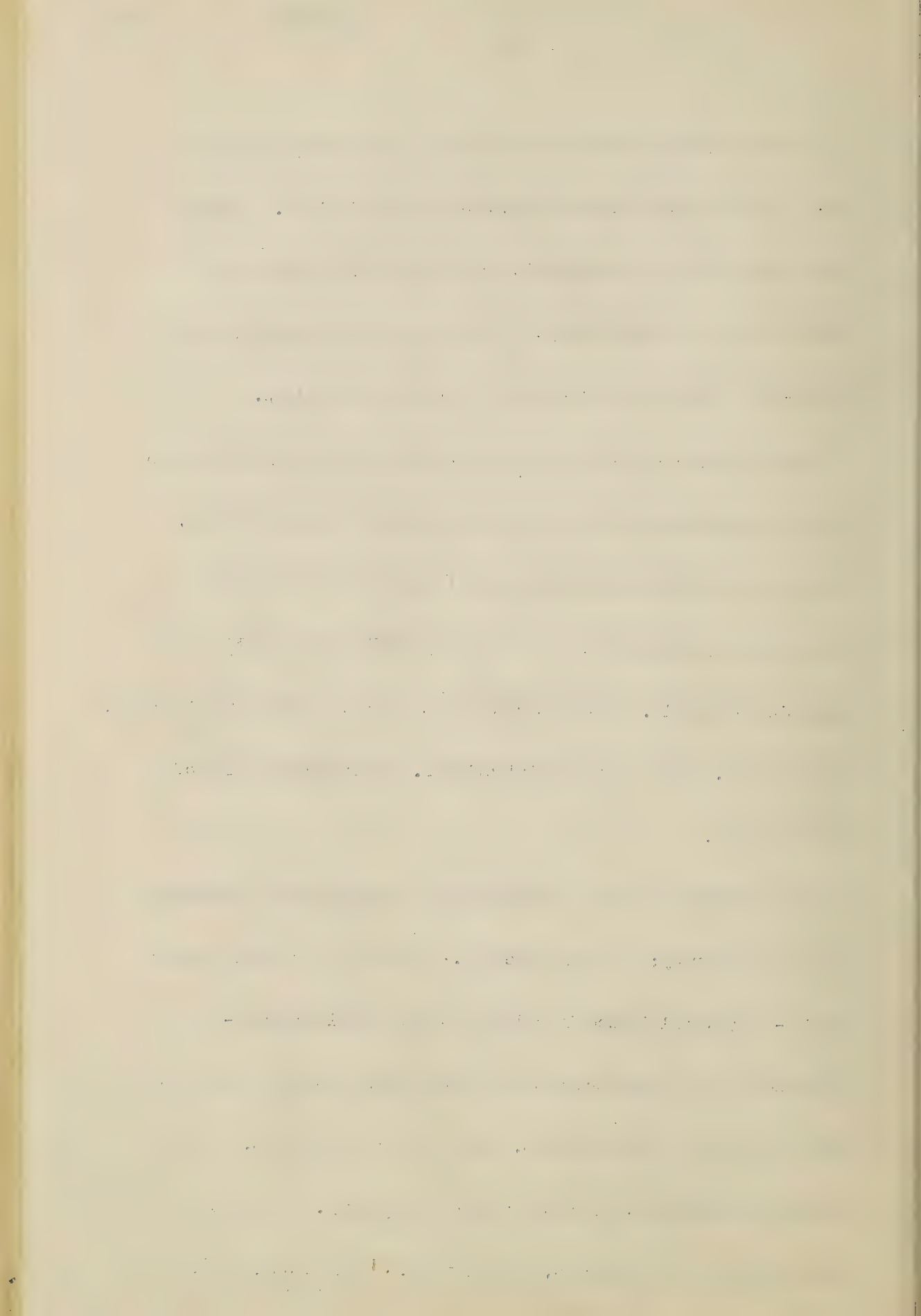
On the 15th I took the train to Wolverhampton as a guest of Whalley Taylor of the British Advisory Service at the Center. I had a good go at the advisory program and the reclamation of farm land that had been torn up with open-pit coal mining. I had one good day with the soil survey party headed by Mr. Mackney. He was doing a good job but with no interpretation. Mackney had seen the need and wanted to do it but he was told to forget it. The work was excellent but useless without interpretation. This was near the end of the British Soil Survey. I had worked on Muir to emphasize interpretations but to no avail.

On September 18 I went by train to London, and then took the plane to New York. I got into my berth about 11:00 p.m. after we left Iceland. The plane was late but I made fair connections and was home again about 3:00 p.m.

I knew I was very tired but this time I had pushed a bit too far. I went to bed early and then awoke about midnight. I knew I was home but had no recollection of anything since being in Taylor's home in Wolverhampton. This was a curious feeling. After a few hours the amnesia left me and I got some more sleep.

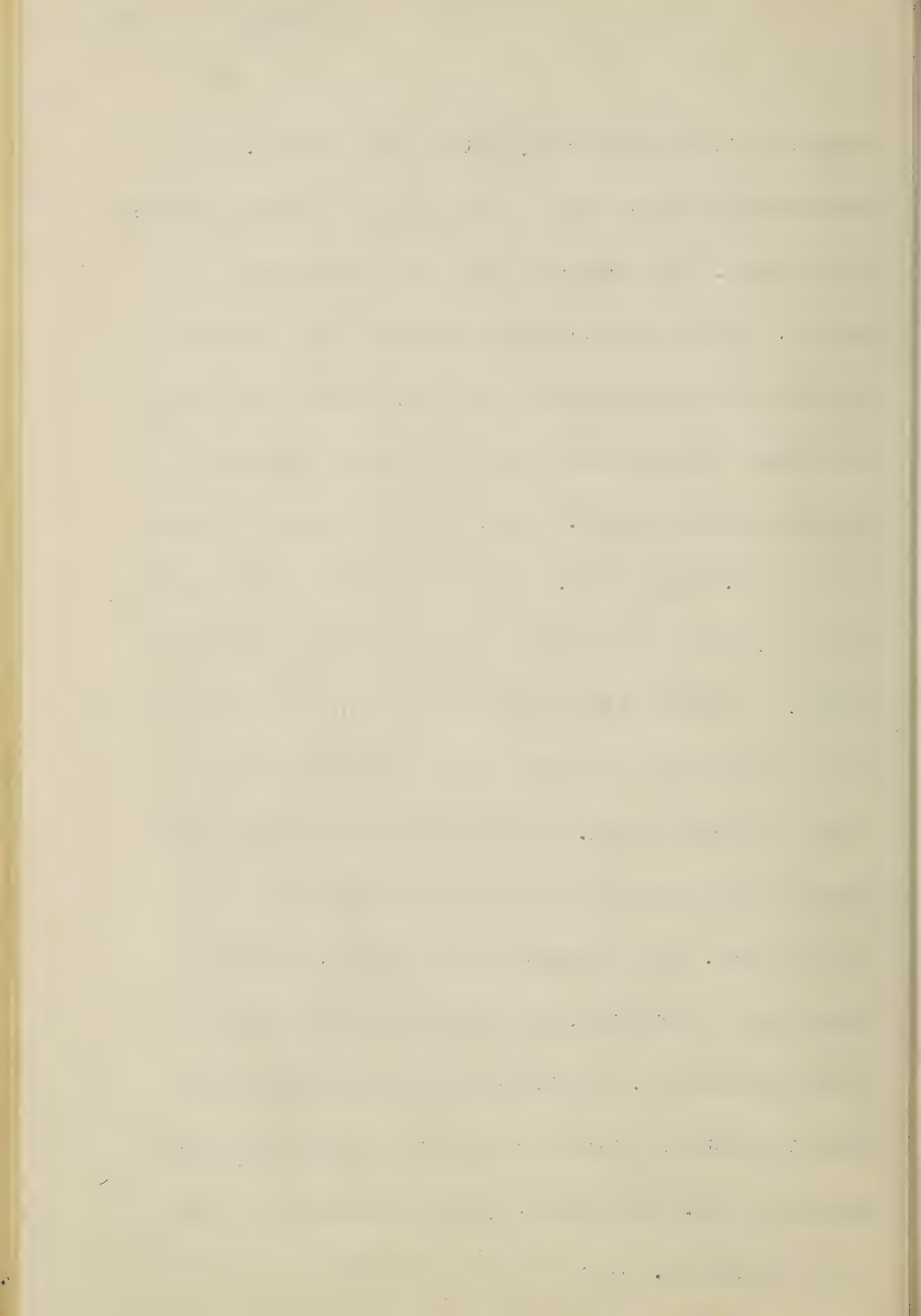
Shortly after returning to the office I learned that Luis Bramao had been appointed as soil survey officer under Ignatieff in FAO. He was so much better than most other Latinos that most of us overlooked his weaknesses. At first he did ~~very~~ well in his selection of people. But he turned out to be a very poor administrator, undependable, and an eager social climber. He caused us a lot of trouble later.

Very rapidly the state conservationists were becoming interested in the Soil Survey; but some were not. Since we had a small senior staff - very small indeed in relation to the total program - I insisted that we concentrate the efforts of this staff in those states where the state conservationist, the state soil scientist, and the state experiment station wanted a good soil survey. The state conservationists in Colorado, Pennsylvania, South Dakota, Utah,



Arizona, and in a few other states couldn't have cared less. By concentrating on the good ones we established some reasonably good goals for the others. This turned out to have been a very fortunate decision. Eventually they all came around because their colleagues in other states were doing so much better they couldn't stand the competition. The state conservationists of Texas and California had dragged their feet badly. Neither had a good state soil scientist for this work. I spoke to Mr. Williams about these two and he explained the policy! First Texas and later California decided to have a good program. In a few years Texas had one of the best.

I took what time I could steal from other things in October to dictate the African Journal. In the meantime I was getting detailed reports from the principal soil correlators and especially from William Johnson. He had the most difficult problems, the least well-trained soil scientists, and the most prima donnas among the state conservationists. But Bill also had the most administrative talent in addition to being an excellent scientist and having a good general education. Thus actually, the work progressed there nearly as well as elsewhere. *As I looked back on it later*



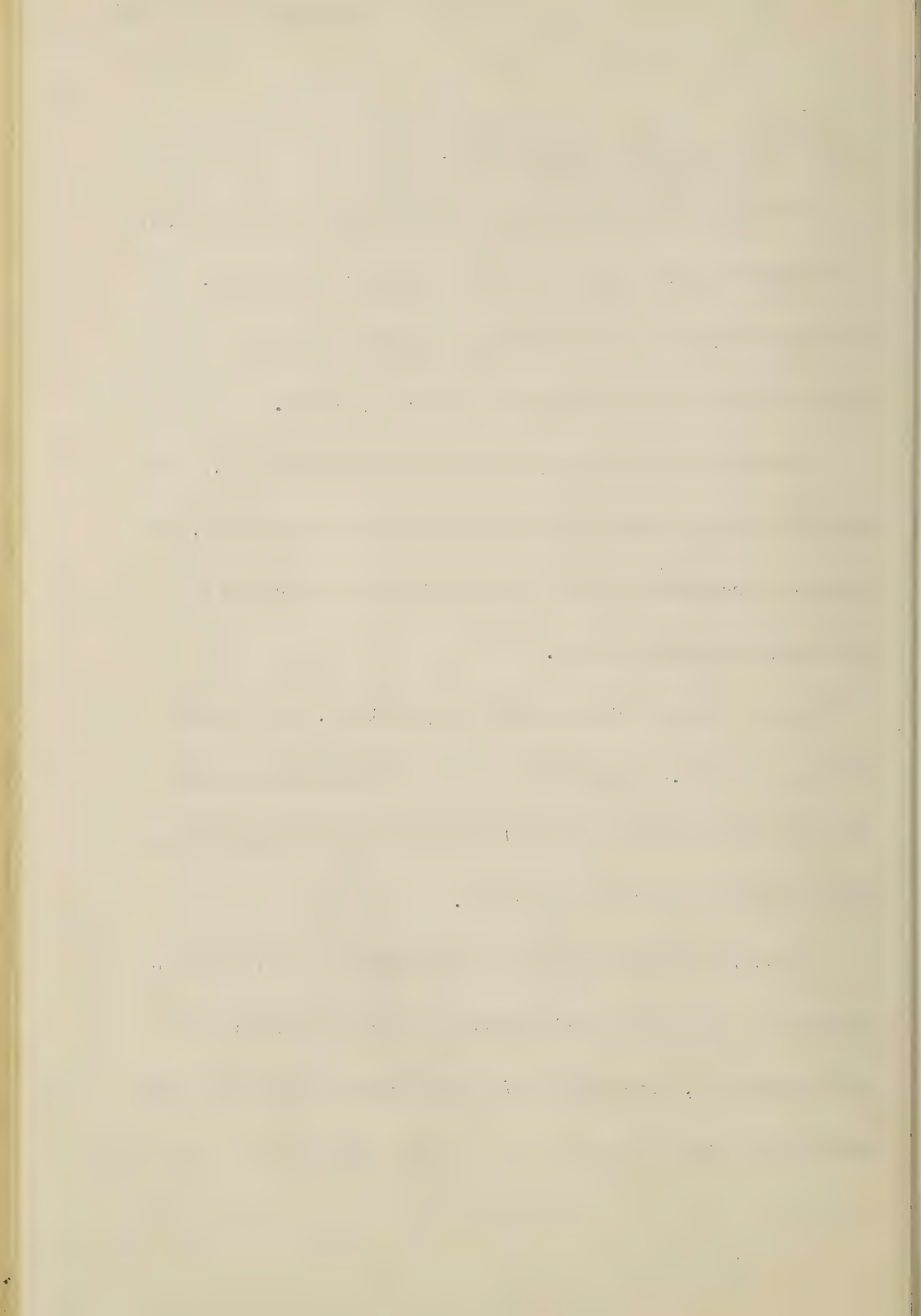
I don't quite see how he managed so well.

Despite short periods of illness that none of us took seriously, I urged David Gardner to go to New York as state soil scientist. He was the most promising young man that we seemed to have and I wanted him to establish his own reputation apart from my office.

Robert had to go for his military training on October 12. He was assigned to Camp Gordon in Georgia and Joan came to live with us. We liked this arrangement because it gave Lucille and me a chance to get really acquainted with her.

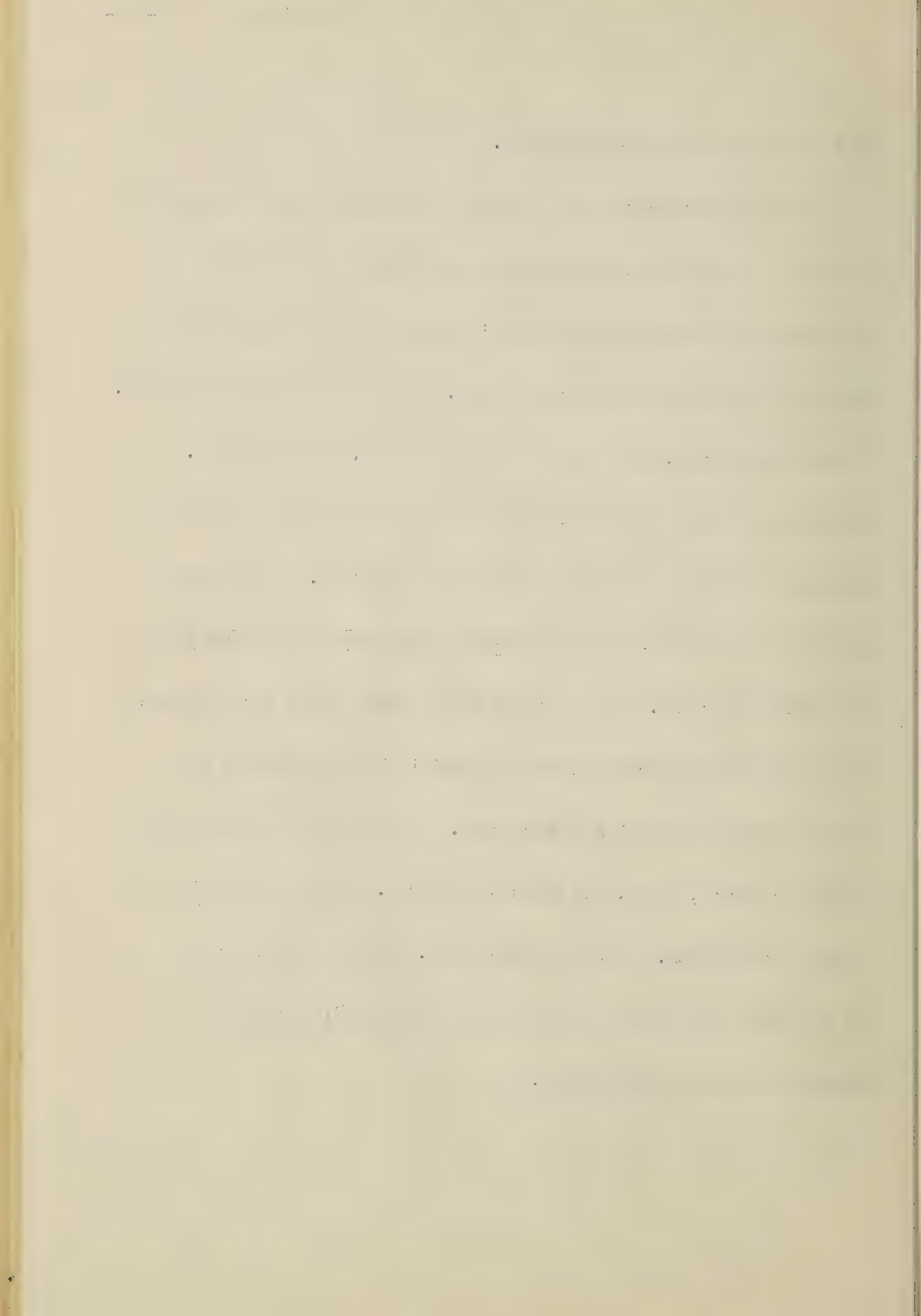
We had the Soil Science Society meetings in St. Paul November 8 through the 12th. I spent some of my time attending the sessions but quite a bit of it in side conferences with the SCS people and the state people on our Soil Survey program.

About this time too Bradfield and the staff at Cornell tried to persuade me to come there for a semester to teach a special course on soil conservation, developing in detail the principles outlined in the address I had given at Leopoldville. But six months away from the



work then seemed out of the question.

We found ourselves with a difficult problem with the Cartographic Division. Don Williams, Gladwin Young (our "general" expert), and Van Dersal all claimed that they didn't need so much money now that they didn't service the regional offices. Orsini insisted that they did. I asked Carl Dorney, our very good budget officer, to help me out. With Dorney we worked out a tentative plan for maintaining complete production cost records for all items in cartographic. Then Orsini and Dorney went to ^{the} Ft. Worth Cartographic Unit where they tested and perfected the scheme. When this was done we made a plan for the scheme to go into effect immediately and for Orsini with someone else to make a thorough inspection of each unit. I went to some of them and I got Van Dersal, Young, and Dykes to help out. "Let us find this loose money" I asked them. None of us found any. It was a little rough on the units but I heard no more of this loose talk. Orsini had been right and the others wrong.



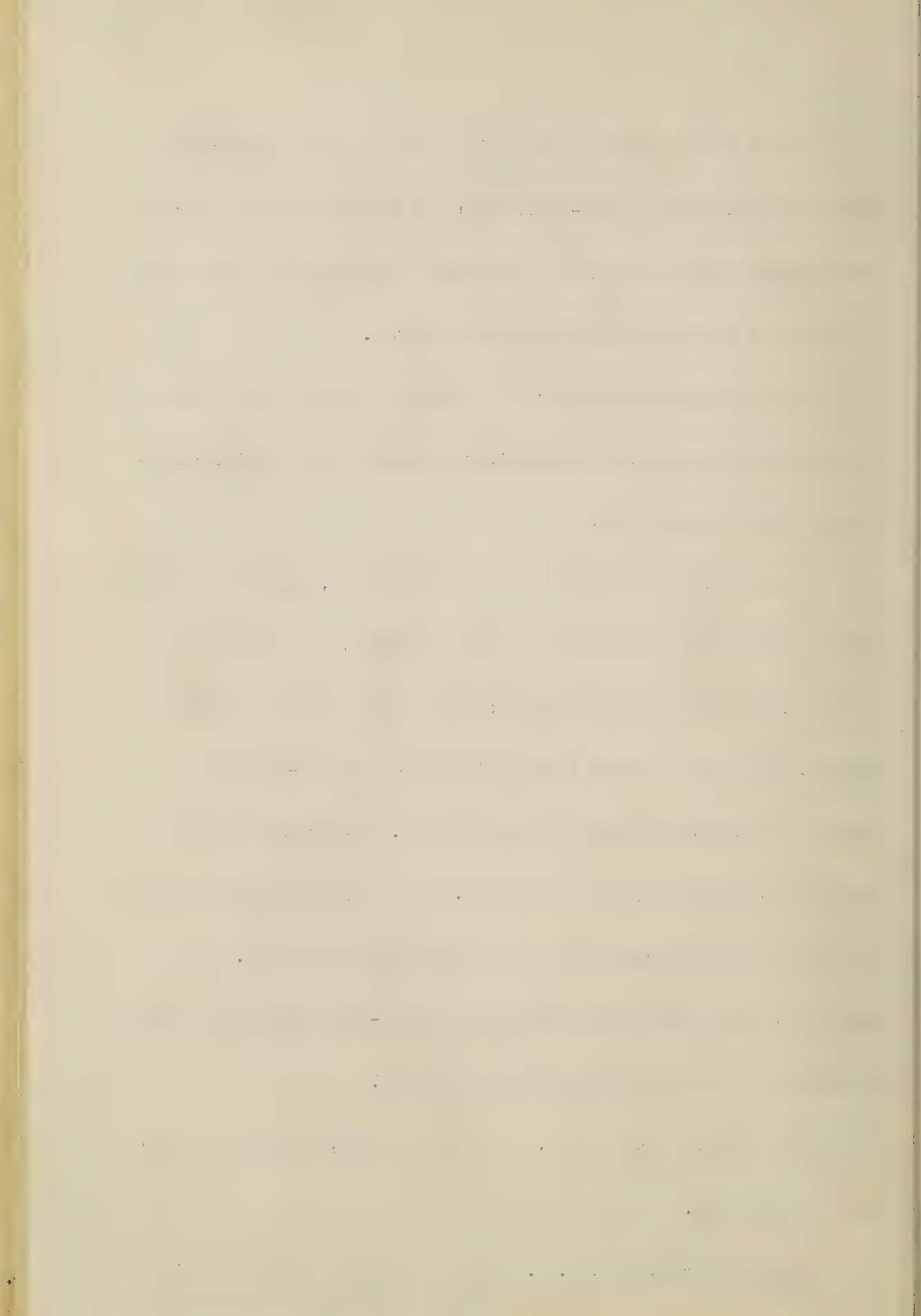
Between November 28 and December 1 I went over the cartographic unit at Milwaukee with a fine-tooth comb. At this inspection we worked out a scheme that saved a lot of floor space by getting rid of thousands and thousands of the old SCS negatives and photos.

In the afternoon of the first of December I talked to the Cornbelt state conservationists meeting in Chicago and had a good talk also with Charles Hardin the next day.

Then on the 6th of December I went to Galveston, Texas for a meeting with the state conservationists of the Great Plains. The sessions that afternoon and the next day were about as dull as any I ever sat through. The various "plant technologists" and other co-called "experts" gave extremely dull and poor reports. The program for the morning of the 8th was just about as bad. But to my delight and surprise we had an excellent session on the Soil Survey that afternoon. I found that these people were unhappy with the so-called capability system and wanted me to get to doing something about it!

I got home late on the 9th, cleaned up my desk, and made ready for another assignment.

Beginning December 13, J. C. Dykes and I made a field and office



review of the SCS program in Maryland. We visited three work units, with much of our time in the field, and one area office, besides three days in the State office. Some of the work was being handled in the way that reflected the leadership of the State Conservationist, E. M. Davis.

We made several suggestions, many of which involved training with help from the technical leaders in the Washington office. The soil survey interpretations were too general and were inconsistent with standards and with the planning guides.

This kind of review is an excellent way to find out the field problems of the Service and what things most need doing.

Robert finished his basic training at Camp Gordon and came home the week before Christmas. Since the military had discovered that he knew the differences between nouns and verbs, he was assigned to the Army Intellegience in the Pentagon for the remainder of his service period. He had permission to live with us. This gave him an opportunity to work on his thesis and Joan helped with the typing.

Mary Alice was home part of the time for Christmas and after Christmas Joan and Robert had a few days in Boston.

As the year ended it seemed that we had made considerable progress. The morale of the staff was good except for Roy Simonson. He had said to me so many times, "Maybe I should get another job," that finally I said to him one day, "Perhaps you should. What do you think you would like?" Either Johnson or Orvedal could have carried on the work. Roy never raised the question again.

1955. Especially in January I became acquainted with E. L. Peterson, who had Coke's place as Assistant Secretary the previous November. Although not an intellectual he was the only one in the Secretary's group whom I got to know who wasn't an anti-intellectual. I think he was a Republican because that was the only opportunity he had as a young man in Oregon. I told him that one day. I said, "Had you been born in one of the border states, you would have been a Democrat." He replied, "That's not true. You don't know what I think."

"That is so," I said, "But I know how you act. Had you lived at the time of Mr. Jefferson you would have been on his side and against Mr. Hamilton."

"I'll buy that," he replied.

I said, "That is all that you need to buy, Mr. Pete."

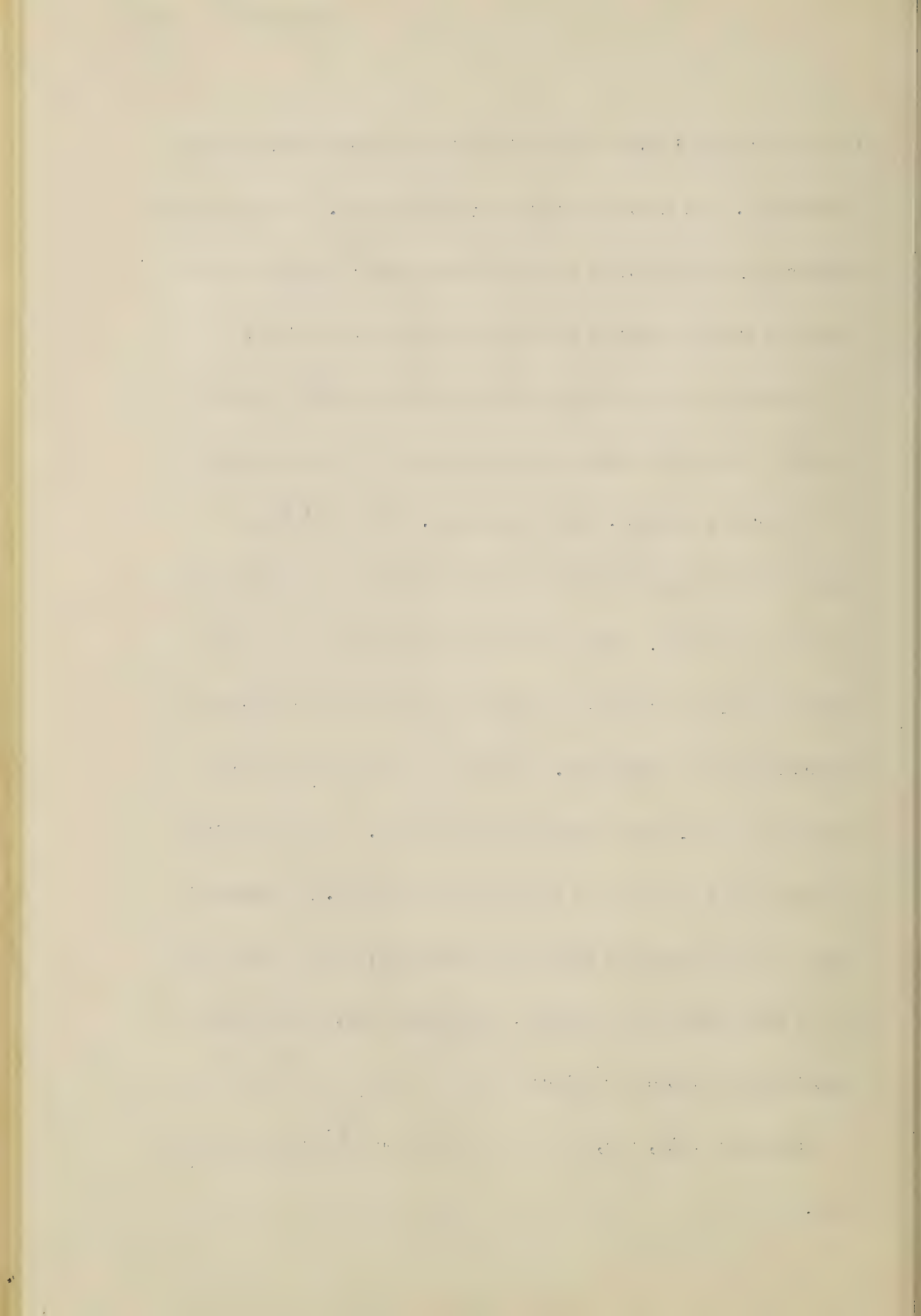
Although I was to get along well with Mr. Peterson, he was to break his word and let me down terribly on one occasion, and for what reason I'll never know.

I spent January 12 and 13 in Jackson, Mississippi for a meeting

of the Mississippi State Association of Soil Conservation District Supervisors. The meeting was not very educational. As the featured speaker they had invited a young man from Spencer Chemical whose education had been made up of Dale Carnegie and Joe Miller.

On the 14th some of the boys from the state office "carried me over" to the state college for a meeting of the local section of the Agronomy Society. Webb went along. He had ~~no~~^t been approved to continue as state soil scientist but had another job in the state office. When we got there and we met the boys from the state college they all greeted one another with exaggerated affection and big handshakes. Yet when the meeting was over Vanderford of the college took me to the plane. As we got going he said, "This man Webb has simply got to be fired." I explained that he hadn't been made state soil scientists, partly because of the objections of the college. So I asked him, "Has he done anything to hurt the work since?"

Vanderford said, "No, but he did plenty to ~~emb~~^{em}arrass us before that."

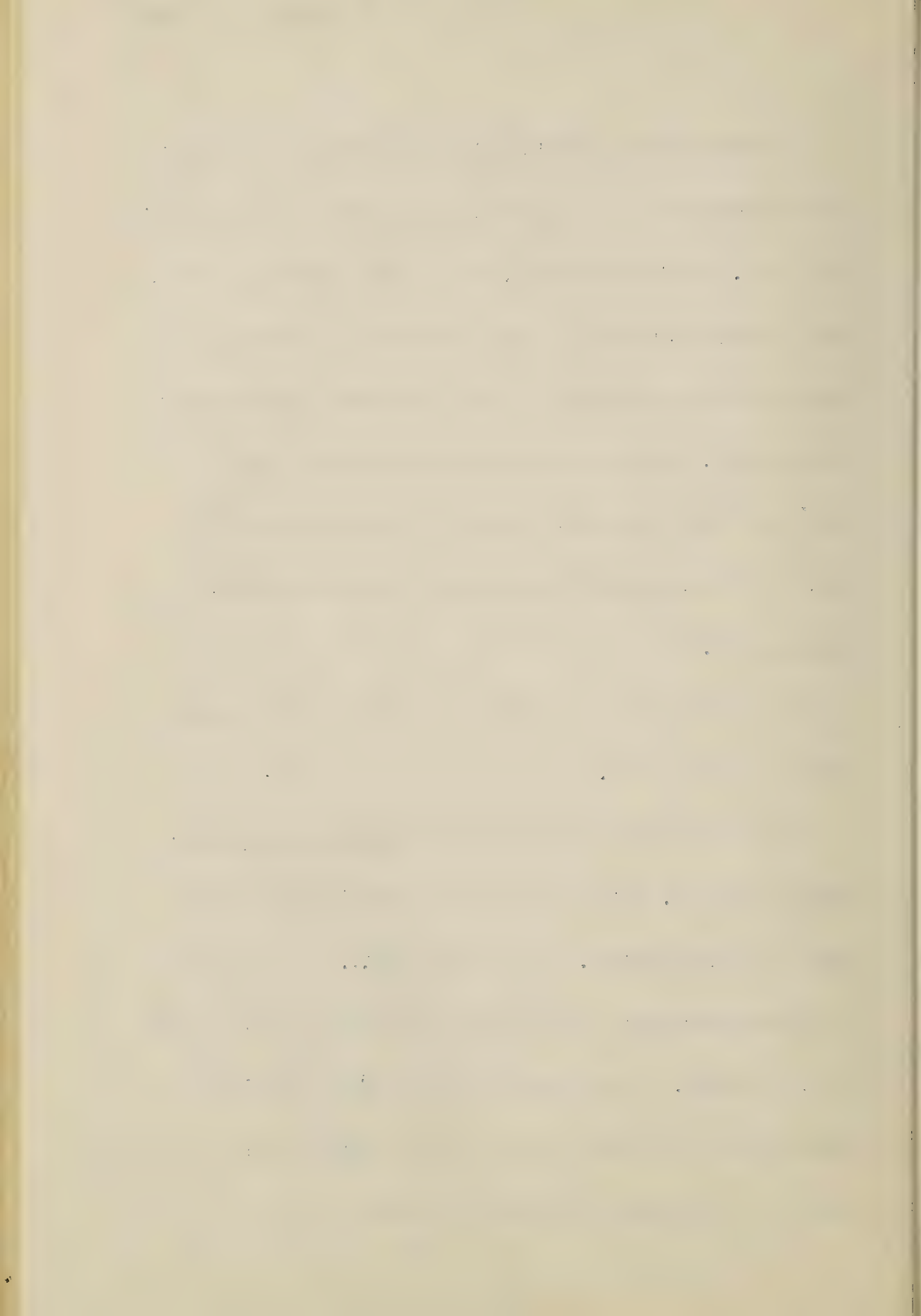


I explained that I couldn't be in any way responsible for what he did before I came in the Service, nor could I hold Webb responsible for it now. I tried to tell him, as I had told hundreds of others, that I simply wasn't going to try to judge the relationships of the past that I was not personally involved with since I had no way to get the facts. Furthermore we didn't have time now to worry about what might have been if Bennett had been a wise and able man. We had too much work ahead of us to realize the current opportunities.

This was a curious conversation in light of the affectionate greeting in the morning.

I gave a speech at this meeting on the Changing prospects for soil conservation. This carried the same positive theme as the one given in Leopoldville. It seemed to go well.

Correspondence was heavy in January and February about the visit of C. F. Charter. It had been agreed that he ~~sh~~^hould come to the United States but we wanted him here by the middle of March so he could attend the National Soil Survey Conference.



In January Alfred Stefferud, editor of the Agricultural Department Yearbooks, talked with me about revising Soils and Men, the 1938 Yearbook. I didn't want to do this since the size of the Yearbooks had been reduced to a point where it just wouldn't be possible to cover the whole broad field of soil science in one book. Yet for three or four years Dr. Salter and I had discussed the urgent need for a book on soil management for use especially by Extension and SCS. In fact, he had urged me to write one but there simply was no time.

Stefferud asked me to put all this in a memorandum, which I did along with the further suggestion that since the bulk of the soil management research had been done at the state colleges they should also be represented on the Yearbook Committee.

This idea for a Yearbook was accepted but I was told that there was no policy for having state people on the Committee. But I insisted that I couldn't be chairman unless they had at least one from each of the four land-grant regions. And it was agreed.

Although a good editor, Stefferud was a master at irritating people. I prayed the Lord that I should not lose my temper with him. The prayer was heard but the answer was, "No". Yet finally we did get out a useful book although not so good as Soils and Men. For one thing, I had been able to read each article for Soils and Men carefully both before and after editing. I did not have the time to do so much with this one and a few errors were uncorrected. During all the writing, and outlining, and proof reading, which took two years, the title was Soil management. At the very last minute, without consulting anyone, Stefferud changed it to Soil. This made the title misleading as well as inaccurate. But other problems were worse.

The following college people served on the committee: Wynne Thorne for the Western States; William H. Pierre for the Central States; Eric Winters for the Southern States; and Nyle C. Brady for the Northeastern States.

As an inheritance from the old SCS, we had many unproductive positions called "area soil scientists." These men felt that they

were some kind of administrators. Since they were to check on the work of people of lower grade they weren't supposed to map soils themselves. These were GS-9 positions and we had a great many of them. Then too, the SCS was a very "rank happy" organization, and the general tenor of the job descriptions suggested that a man's grade depended on how many people reported to him. To make progress we had to get these GS-9s on to productive work. Thus I undertook to rewrite all of the job descriptions for soil scientists in SCS so that the number of people reporting to the incumbent - whether none or 200 - was not an allocating factor in any of them from GS-5 through GS-15. This was quite an undertaking because of the inefficiency of our personnel people but we finally got it done, and these new standards came into use four years later. In the meantime we got rid of many of the positions.

This did a great deal to increase the amount of acceptable mapping per individual. The state conservationists had been complaining that all the soil descriptions, reviews of field work for soil correlation, and soil survey reports were slowing down the mapping.

Yet the record showed the reverse. I commonly explained that the reason was that the men now had better training, better guidelines, and more interest in their work. These were all true but I knew myself that getting this great group of GS-9s into productive work had a great deal to do with it.

During this late winter and continuing through the summer I had much correspondence with soil scientists abroad and especially with Alex Muir. The British governors in the colonies and territories were trying hard to get United States money. They kept putting through projects about soils that the soil scientists in Britain never heard of even though the Colonial Office established a position at Rothamsted for this purpose, which was occupied by Herbert Greene. I found that Norton had been promoting a soil survey in Fiji with

FOA a/ financing. I had thought to have scheduled this for the New Zealand

a/
When the United States started the Marshall Plan, and for a long time afterward, officials of the government emphasized its "temporary" status. After the massive effort in western Europe, aid to those countries did diminish but anyone who had studied the problem at all knew that the United States would be giving aid of one kind or another to the undeveloped countries for a long time. The United States organization went under many names: Marshall Plan, Point Four, ECA, TCA, FOA, and AID.

Soil Survey



This was a busy spring. We had made gains in the Soil Survey organization but had a great deal to do. More advanced guidelines went to the field. We decided to have a meeting of the 1957 Yearbook Committee in August during the Soil Science Society meetings at Davis, California. It was necessary to have some firm alternative suggestions in advance for the committee to react to. Dr. Salter was on the committee but was unwell. Dr. C. P. Barnes was especially helpful. Soon after Barnes moved to ARS he did not have enough professional work to do. I discussed this briefly with Shaw and asked him if he minded if I asked him to some things in his professional field. Shaw was agreeable and Barnes helped me a lot with problems in geography.

Also I had several speeches coming up and a very heavy travel schedule. I did as much of the writing in my office as possible to avoid doing it on trains and in hotels.

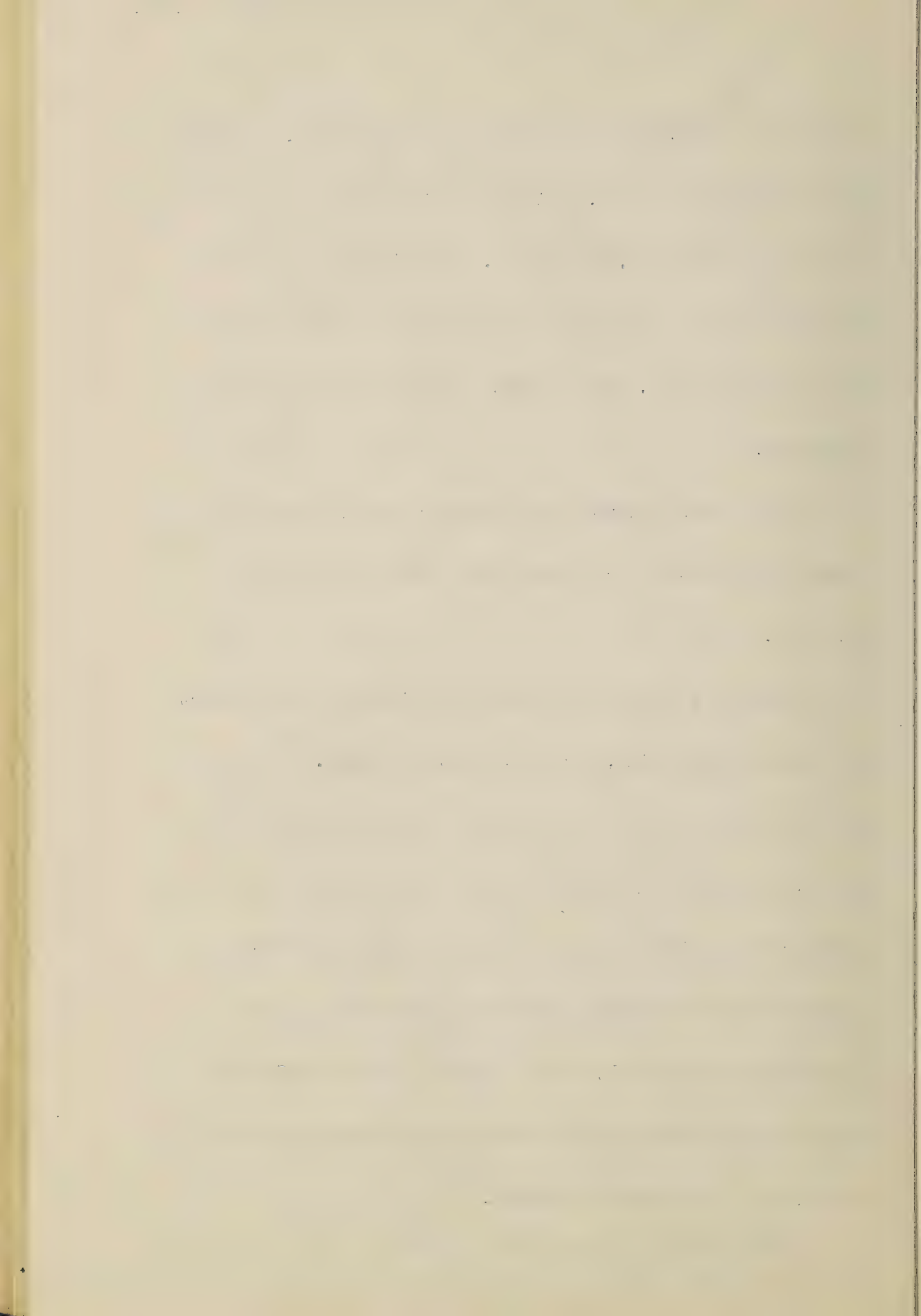
C. F. Charter of the Gold Coast came about March 15, He stayed at our home for a few days. Lucille and I met him at the airport

and discovered that his bags had gone on to New Orleans. I loaned him pajamas and a bathrobe. This took a bit of turning up since he was somewhat smaller than average. Here he talked with all of the soils staff in SCS and ARS and planned his excursion against his leaving August 18. This included attendance at our National Conference.

A little later Professor Rene Tavernier came to visit with us and also to attend our National Soil Survey Conference in St. Louis.

On March 23 I spoke at the Medical Association of the District of Columbia on Our soils, our food, and time to live. I had a good question period but the informal one afterward surprised me no end. A considerable group of these medical doctors were "organic gardeners" and tried to get me to agree that if one used only compost (or the Howard-Rodale type) the plants would not suffer from diseases and insects! One is amazed at this nonsense from anyone but from people with the biological training required for an M.D. he is considerably astounded.

Again that year we held the National Soil Survey Work-Planning Conference in St. Louis



March 28 to April 2, 1955

To a considerable extent the National Staff Conference ~~this~~^{year} was an extension of the one a year ~~ago~~^{earlier}. We still faced the most difficult problem of training. At the beginning of the conference I highlighted some of our other problems.

I discussed with the group our problems in soil survey interpretations at some length. First of all, we still lacked precision that depended on better descriptions and better yield estimates. We lacked any scheme for orderly revision of the soils grouped into capability units and classes. Many of the existing groupings were hopelessly out of date. We needed a system that involved writing down the actual facts prior to interstate meetings.

At the beginning of the conference we had summaries from each of the regional conferences that had met earlier. Some of the particular items taken up were the following:

1. ^{the} A committee on soil moisture progressed a bit beyond ^{the previous} last year but it became obvious that we should need to have some competent climatologist work with us.
2. The training committee did not make much progress ^{but} we did have a useful discussion.
3. The classification and mapping of eroded soils was further considered. Essentially, the recommendations came back to the ^{2nd edition of the} Manual.
4. We had a committee and a good discussion on improving the effectiveness on the use of soil surveys both before publication within districts and especially the interpretations for the use of the published soil surveys.

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5. As before, a good deal of attention was devoted to the new system of soil classification and specifically to the Fourth Approximation of the scheme.

6. The committee established ~~last~~ ^{the previous year} came forward with a uniform set of symbols for field maps but it took another year to get this ~~straightened out~~ ^{completed}.

7. Mr. Orvedal gave an excellent discussion on the engineering applications of soil surveys. This had already been developed to a high degree in his World Soil Map Project and we hope ^{it} to spread this ~~skill~~ ^{skill} throughout the Service.

On the whole this was a bit more orderly meeting. It was obvious to me that we had a very great deal to do on soil survey interpretations and especially on the so-called "land-use capability system." (Unhappily, this was not really a system.)

Besides our senior staff and a few State soil scientists we had representatives from the four land-grant regions, two of our associates from Canada, Downes from Victoria, and Charter from the Gold Coast. Charter obviously enjoyed himself a good deal. Arrangements had been made in advance for him to have field trips with many of our people immediately following the work-planning conference.

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2. As before, a good deal of attention was devoted to the system of soil classification and specifically to the Fourth Edition of the scheme.

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4. Mr. Luedel gave an excellent discussion on the applications of soil surveys. This had already been developed in a series in his soil map project and we hope to report this all throughout the service.

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After the meeting in St. Louis I had a bit more time in my office, which I sorely needed, for the Yearbook and speeches.

I went by train to Albuquerque, New Mexico and first inspected the Cartographic Unit there. I was amazed to see that some of the ~~maps~~ ^{maps} from the field still had only the capability units without the soil lines. This obviously had to be changed. Many of these western states were using ridiculously long fractional symbols. (It took years to get rid of those.) I came to the conclusion that we could abandon this office without any great harm.

On the evening of the 26th I attended a banquet and open meeting of the International Arid-Land Symposium sponsored by UNESCO and AAAS. ^{a/}

^{a/}

The papers and recommendations (but unhappily not the debates) were published by the AAAS under the title, The future of arid lands. Special publication No. 43, AAAS. 453 pp. illus. Washington, 1956.

Georges Aubert came to this meeting. It was very good to see him again. Although quite old then, ^{Dr} Shantz was there and gave a paper.

At the conference I looked at the most beautiful movie I had ever seen, The living desert, a Walt Disney production. I understood that a man- and wife-team made it. They must have had several times the patience of Job. Then after it was photographed a clever musician selected or composed music to fit the movements of the animals. This made some parts very funny indeed.

I gave one of the introductory papers on which I had spent many a lonely hour, The role of science in man's struggle on arid lands.

On April 30 I went by plane to Nashville, Tennessee. This was Saturday and I spent Sunday in writing.

Beginning Monday, May 2, and running through May 6, with the help of C. H. Dorny and J. J. Coyle I made a review of the Service work in Tennessee. We had some interesting field studies.

The soil survey interpretations in some of the older counties were felt to need updating badly but the state soil scientist had wisely decided to concentrate in the beginning on the new surveys.

On the whole the work in Tennessee was very good, especially staff relationships, which reflected the administration and drive of the state conservationist, J. R. Sasser.

The state was fortunate in having many more published soil surveys than most states but the interpretations of these needed to be updated. More competent help was needed on forestry.

Tennessee had made an excellent start in the small watershed program. They needed the services of a full-time watershed work planning party.

I enjoyed this visit in Tennessee very much. Sasser is an interesting and challenging man to be with and his staff admire him very much. What a shame we didn't have more state conservationists like him.

We made arrangements for Dr. Roy W. Simonson to go to Brazil for a period under the auspices of TCA to help them get their soil classifications and surveys under way. Luis Bramao had tried to get them started earlier. Much as I hated to have him away, Roy was there from May 15 to July 13.

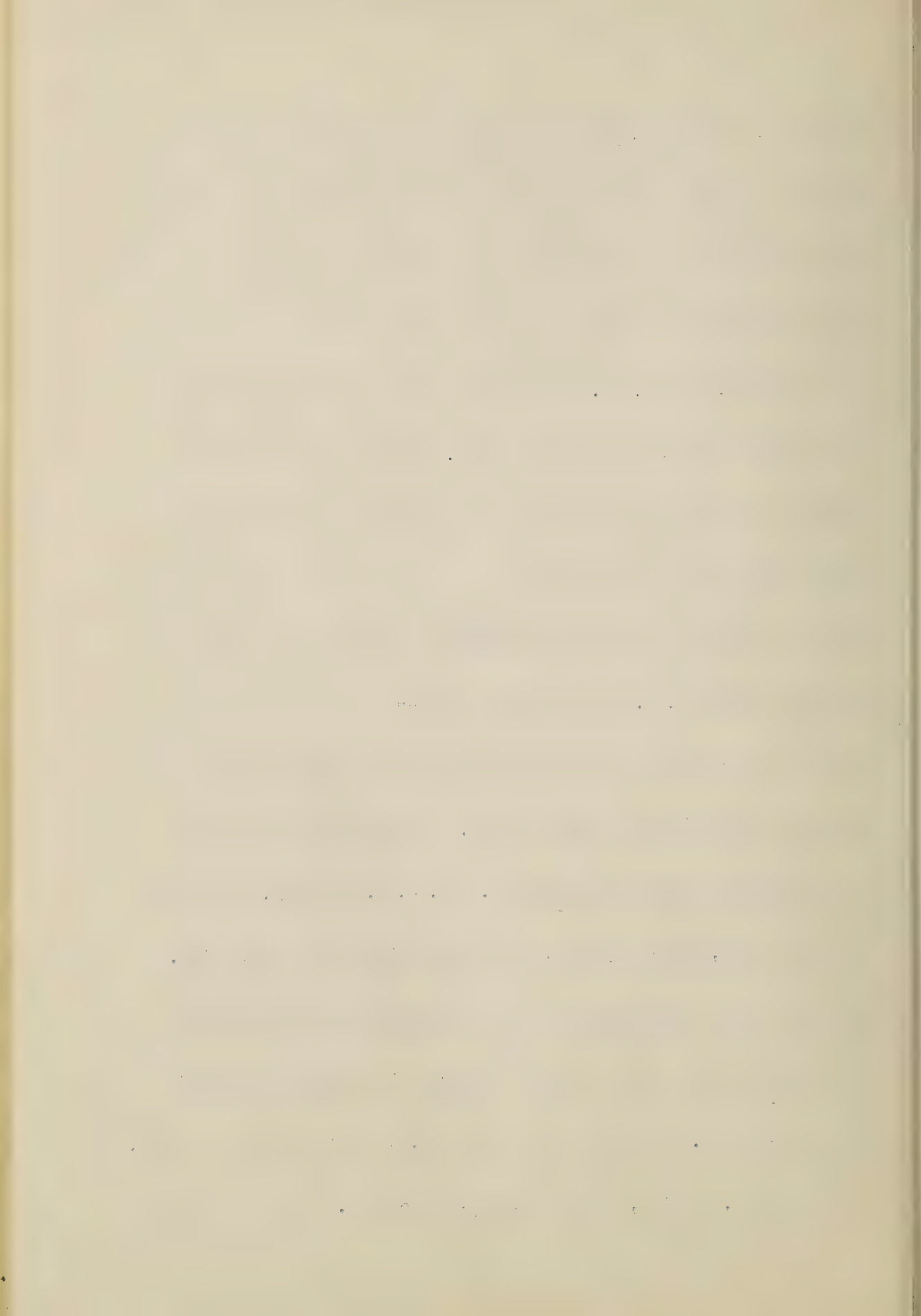
May 1³ and 14 were spent with Dr. Black and his group at Harvard including one evening with Mary Alice. An old SCS soil scientist by the name of Tom Day was finishing a year there. His wife seemed to be very intelligent but he impressed me as a little shifty.

Later I arranged an excellent opportunity for him in Tennessee with Ralph Sasser. But he flub-dubbed it and went with TCA. I have never liked to think of the kind of work he probably did abroad without the supervision that he greatly needed.

On Sunday, May 15, Dr. Black took me out to the Harvard Forest and we had a good go with Director Raup. We walked in the woods and looked at the soils and their interpretations. That evening I took the train for Bangor, Maine.

May 16 to 20 I made a program ~~review~~^{review} of the SCS work in Maine with the help of T. B. Plair and L. E. Lyman.

The need for area conservationists between the state office and work units was highly questionable. The staff needed guidance on work-load analysis and management. Mr. J. S. Hardesty, the state soil scientist, was weak, slow, and unproductive with his own time. Much more needed to be done on soil survey interpretations but the staff was so small that the state soil scientist should do much of the work himself. Both Hardesty and Oliver, the state conservationist, liked to hunt, to fish, and to camp a bit too much.



I used to kid Oliver about this. I told him, "The sure way to get more Service work done in Maine would be to prevail on the Governor of the State to cancel the hunting and fishing licenses of the state soil scientist and the state conservationist."

According to my old friend Joe Moon this was a bad statement. He insisted that one should never joke with the truth. (We finally had to relieve Hardesty of his responsibility; and Oliver was sent to Alaska.)

This program review took a lot of work and a lot of digging to get at the facts. One of the work-unit conservationists in Aroostook County resented some of my questions and became a bit surly. I said to him, "You have your job and I have mine. We are going to get along fine if you answer these questions that I'm asking. Otherwise I don't know." After that he was a model work-unit conservationist.

On May 25 I flew to Memphis for a session with the state SCS staff of Tennessee. Mostly I talked about Soil Survey problems, especially interpretations, and our work with low-income farmers.

June 2 to 4 I had an interesting session at Louisville, Kentucky with an interdenominational group of Protestant pastors interested in better living standards for farm families with a good deal of emphasis on soil conservation. Both Congressman Hope of Kansas and Hayes of Arkansas spoke at length but seemed to say very little.

I helped the pastors the best I could with definitions for their *2 gave paper on Family Farm or Family Farms* reports. These pastors couldn't avoid emphasis on almost pin-point arguments about theology. It certainly became clear that one could help them only with simple statements. Each one would want to handle the theology a little differently.

Sunday morning I did some writing and then took the train to Nashville.

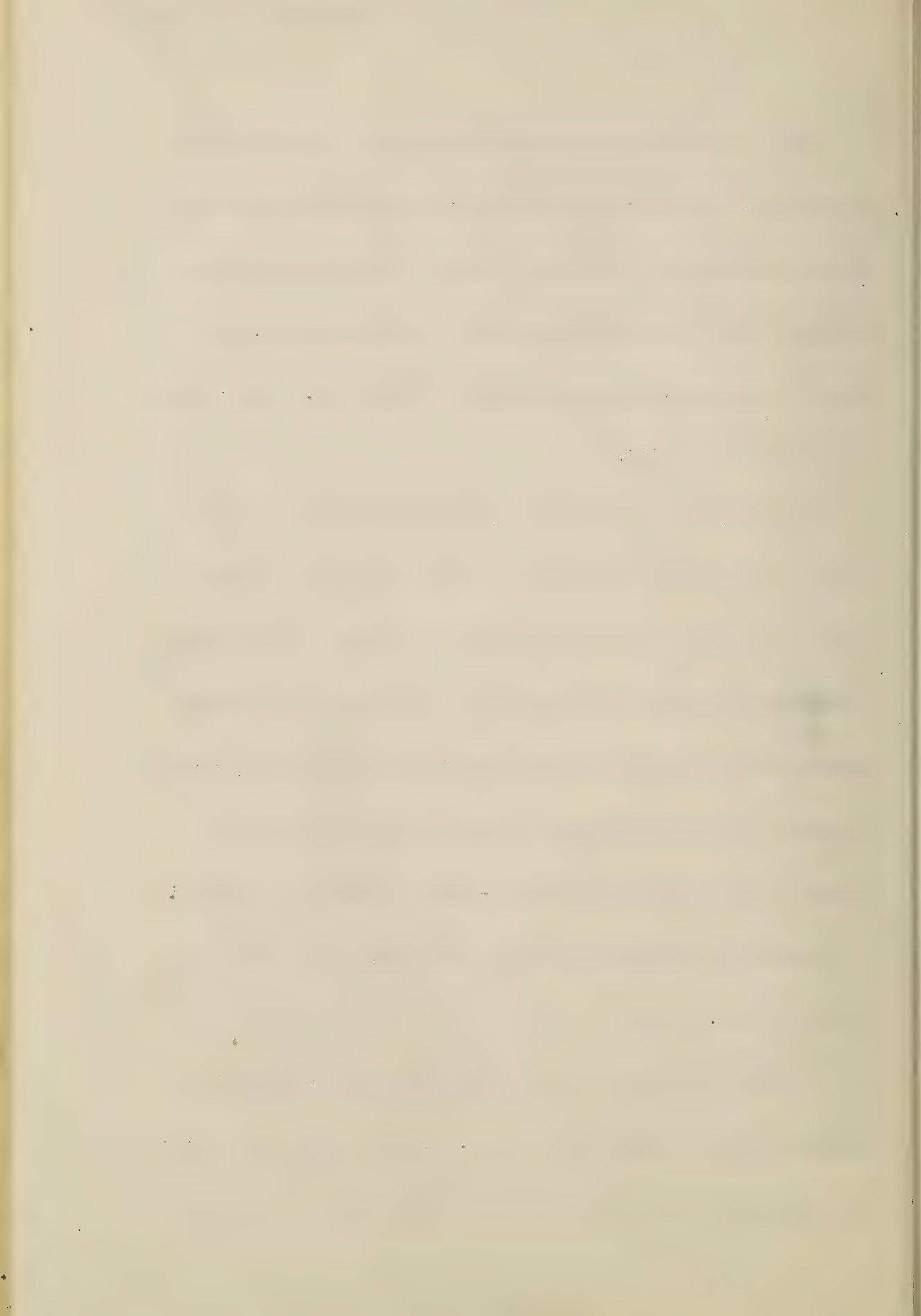
Monday, June 6, Sasser picked me up and we drove down to look at the Ames Plantation. This large old plantation had been purchased by a wealthy Bostonian as a bird-hunting refuge. Nothing much had been done with it beyond maintaining the big house. Dr. John D. Black knew him and persuaded him to leave this to the University of Tennessee to be used for experiments with different kinds of farm units.

Here I met an interesting young man trained in forestry, Solon Barraclough. He was bright and hard working but a bit liberal on the race question for this area. Land was being cleared and farm units laid out. Water control work had been badly planned. The soils were erosive and needed terraces and diversions. Sasser agreed to help them with these.

We had a nice lunch in the big house with some of the people in the University and then drove to Memphis for a big conference on low-income farms. This got underway the morning of June 7 with Under Secretary Morse as chairman. Much of the conference included exactly the same things we talked about in the days of county planning plus some Republican nonsense. A Deputy Assistant Secretary of Commerce even suggested training low-income farmers to be salesmen!

Generally the extension people suggest only more of what they are doing.

It was perfectly obvious to me that the southern directors of extension were not buying this program. Although many white farmers had low incomes, essentially all of the negroes did. Thus if they



redirected their work toward the low-income farmers they would be emphasizing the areas with heavy negro population. And they weren't about to do this.

In June the piece^{about} about the garden soil during the seasons that was published in the "Flower Grower" the previous year, appeared in French as La Terre in "Maison et Jardin", published in Paris.

That summer Mary Alice was able to arrange a holiday in Europe for a month.

Early in July I had a letter from Ignatieff that now FAO was prepared to develop a second edition of The efficient use of fertilizer.

This had proved to be an extremely useful book in the newly developing countries. We had some further correspondence about it and I gave him all the ideas I could think^k of and later sent him our prospectus for the 1957 Yearbook on soil management.

On July 12 I had a talk with Don Williams about Henry Adams who seemed to be serving unsatisfactorily as state soil scientist in New Hampshire. He wondered if we could get some service out of him as soil correlator for interpretation in the Northeast. We

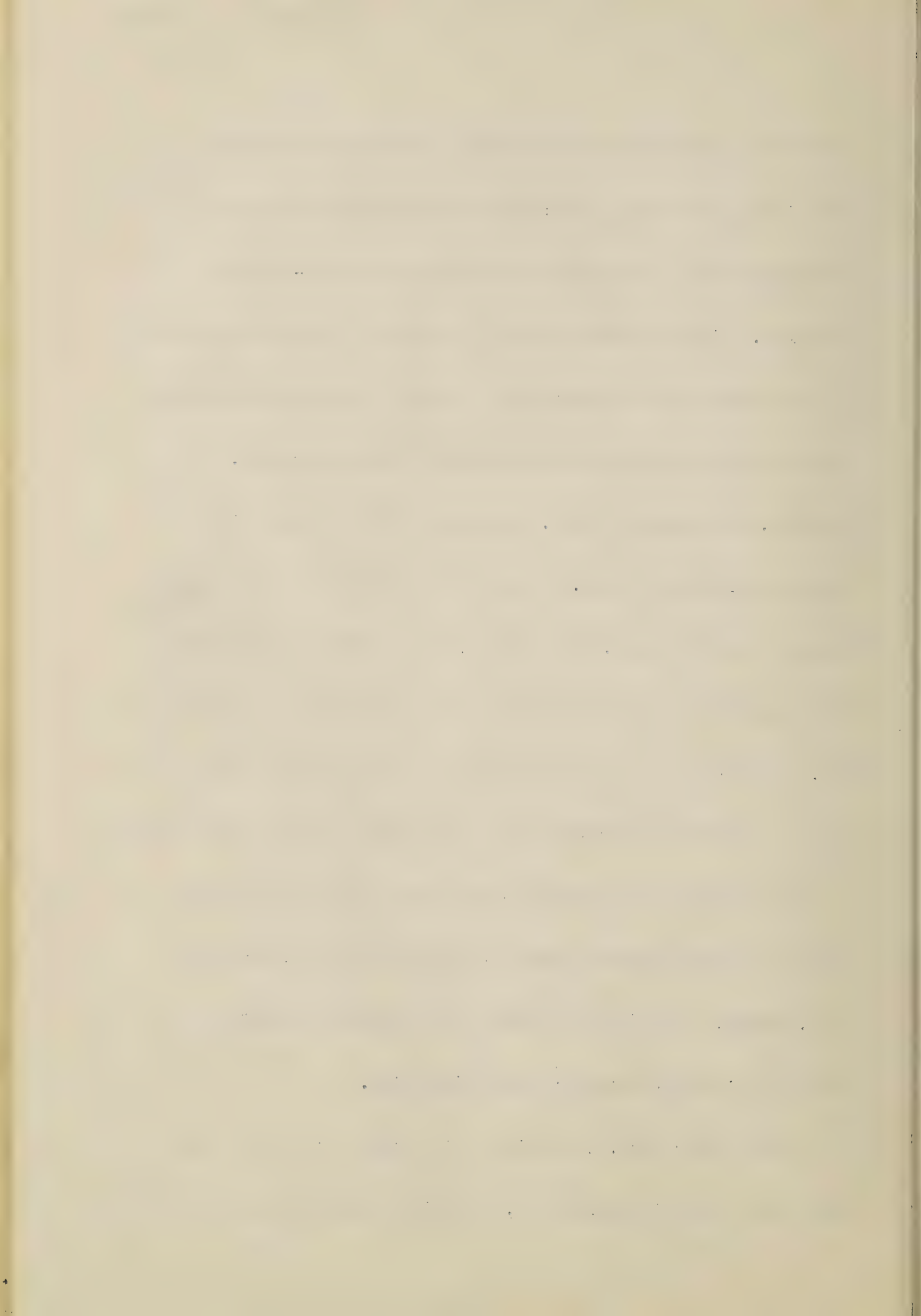


also talked about other possibilities. Williams said he would leave it to me but that I should need to find a place for him. I explained that he could probably not do more than one-half of a work load. Williams agreed but still insisted I find a place for him.

Adams must have had something on somebody because he was clearly a poor employee and should have been fired for incompetence, laziness, and neglect of duty. Instead, we were to bring him into the Washington office, protect his grade (GS-12), and let him "retire" on full salary. But even after this Williams, VanDersal, and Mohagen spoke seriously about the wonderful "career policy" in SCS! The next day I called Arnold Baur. He told me that Adams would be useless on correlation but might help a little on interpretation.

From the 18th to the 22nd of July I made a program inspection of the SCS work in North Dakota with the help of John Sherman and J. H. Wetzel. We split up for some of the field visits and I started with work around Fargo and Valley City.

I got a fairly good impression of the field work but a rather poor one of the soil survey work, primarily because of the



indifference of Shoesmith, the state soil scientist.

Also Mr. Wetzel reported that the watershed program was lagging in relation to the commitments made and projects under way.

The state office agreed that clear functional guides and better management program were needed. Many numbered and unnumbered memoranda had been issued on both technical matters and policy but no arrangements had been made for following through. Apparently the staff had learned they could ignore these memoranda. Apparently the state conservationist, Mr. L. G. Lloyd, had too many irons in the fire and other interests to keep track of things himself.

Very little progress had been made on the conversion of the conservation surveys even where the men in the field were adequately trained for it. Nor was appropriate use being made of the published soil surveys in farm planning. We made a good many recommendations that would need close follow-through to have results.

While at the state office I looked into the work of Adrian Fox, deputy state conservationist. His whole interest was in wild life and elementary "conservation education". The files showed him to

be incompetent as an administrator. I had just finished that part of the review when Williams called about him. I told him the facts and that he wasn't able to do his job. A bit later he too was brought into the Washington office "to maintain liaison in conservation education, youth groups and the like."

We discovered almost accidentally that the North Dakota SCS people were making "conservation surveys" of scattered farms in McKensie, Billings, and Morton Counties, which had good recently published soil surveys, simply because the early mapping was done before air photos were available. We told them that this waste of funds must stop and explained that if they couldn't train the field people to use the published maps I should be glad to come out and do it for them personally.

We finished up the state inspection of North Dakota with mixed feelings and I went on by train through Chicago and Springfield, Massachusetts to White River, New Hampshire. Here I was met by a young man from the SCS state office and driven to Meriden, New Hampshire where I took part in the Gordon Research Conference on

Biochemistry and Agriculture sponsored by the AAAS, from July 25 to 29. Here a selected group of scientists were expected to live together for three and one-half days and speak frankly about their new ideas without publication. For some reason I was not enormously stimulated by the meeting although it was good relaxation with some intellectual content. I gave an illustrated lecture on the effects of vegetation on soil formation with specific examples.

The government car had been left for me and I drove it to Durham, New Hampshire Friday afternoon. Saturday and Sunday I spent mostly in writing on speeches that I had yet ahead of me.

From Monday, August 1 to Friday the 5th I made the program inspection of the work in the Service in New Hampshire with the help of C. H. Grey. We spent part of our time in the field with Kenneth Grant. On the whole the work looked fairly good.

Henry Adams was there as state soil scientist but actually was not able to carry on the work of that position because of his inability to drive an automobile. We had to recommend that some change be made and it ended up that I had to find a place for him

(He didn't do much more there but at least he was not quite so conspicuous.) But considering the resources he had available Mr. Collins, the state conservationist, was carrying out a good program.

On Friday the 5th I took the noon train to Boston with the intention of having the afternoon for the book stores. But it was just terribly hot. I went to a movie for the airconditioning and was directed to a cool restaurant - Locke-Ober - in a kind of alley. I took the full course from hors d'oeuvres to brandy and allowed a leisurely walk to the railway station. But the train was derailed in the yard so I sat in that terribly hot station, because of the frequent promises that we could soon board, from about 8:00 in the evening until a little after 1:00 in the morning. I was thankful that nothing had happened to the air conditioning.

On August 13 I flew to San Francisco and on the 14th Bill Johnson met me in San Francisco and we drove to Davis for the meetings of the Soil Science Society.

I attended the executive committee meetings and spent most of

Monday in various conferences including a meeting with the Yearbook Committee. Much of the time at these meetings I spent listening to complaints and in interviewing men for various jobs.

We had a second meeting of the Yearbook Committee on Wednesday, August 17th and agreed to have a three-day meeting in Washington about the middle of October to complete our prospectus and the final selection of authors.

Early in the morning of August 18, Bill Johnson took me to the Sacramento airport and I worked my way by air to Lansing, Michigan during the day. The next morning I had discussions at the SCS state office on the campus with some of the college people about the soils work in Michigan.

Luncheon was with Dean Tom Cowden and others. After the luncheon we went to the main stand for the celebration of the centennial of Michigan State University. During the ceremonies I received a citation for Distinguished Service. This was followed by a sort of pageantry play featuring the historical development of machines for farmers. It was unbelievably hot. Shortly after

this concluded I went to my hotel to cool off and rest. The next day I returned home by air with a notebookfull of items to take care of in the office, especially on the Yearbook.

A few days after getting back I went out to the Plant Industry Station to discuss the Yearbook with Dr. Salter. He was in miserable condition but probably didn't know it. He could discuss each item that I introduced very sensibly but only three or four minutes. Then we would need to change to something else.

I used the next several days to get caught up as nearly as possible at the office and with the Yearbook. By an arrangement with TCA Guy Smith spent most of September in Surinan and British Guiana helping some new men puzzle out a soil classification.

September 12 to 14 I attended the meetings of the Soil Conservation Society at Green Lake, Wisconsin. Most of the sessions were a bit superficial and nothing of direct concern to either soil science or conservation. On the 13th we had word that Salter had died. Cecil Wadleigh was very much upset. He said to me, "This means

that Omer Kelley will be director. They (meaning Shaw) should get somebody from the outside."

I agreed with him that they ~~should~~ get somebody from the outside who could make a fresh start. "But," I said, "This calls for more energy and imagination than 'they' are likely to put out. But it won't be Kelley."

"Who do you think it will be," Wadleigh asked me.

"You", I replied.

"Oh no," he said, "Furthermore they ~~should~~ get someone from the outside."

Later I supported Wadleigh for this position because so many people disliked Kelley. This turned out to be one of my serious mistakes.

The next morning I gave a lecture on Soil surveys in modern farming, which was later published in the Journal of the Society.

The purpose of this talk was again the positive one of using the Soil Survey to capture opportunities.

I made a tape recording for the Des Moines station and took the train to Washington.

I returned home the morning of the 15th with a little time to spare before going to the funeral of the late Dr. Salter. Salter would have lived quite a bit longer if the Secretary's Office had treated him decently. It would have been kinder had they simply shot him in 1953.

Saturday evening September 17 I left for Lincoln, Nebraska and got there Sunday evening for the first annual meeting of state conservationists. Williams gave a good opening and each of the assistant administrators - Dykes, Van Dersal, and myself - made short general statements. At these meetings most of the work is done through committees.

On September 20th the whole group took part in a field trip to look at some of the work near Lincoln with special emphasis on water control. That evening Joe Casey, Head of the Lincoln Cartographic Unit and his staff put on a wonderful exhibit of their work. They couldn't have done better. I think Williams was quite impressed since after that I had less trouble with him over cartographic budgets.

At that meeting, and at the others which followed, I had a good many very helpful side conferences with individual state conservationists about our work.

I recall that at that one I made arrangements with Robert Boyle, state conservationist for Arizona, to let me have his state soil scientist, Roger Headly, for a year of training in the position as Assistant Director of Operations left vacant by R. O. Lewis. Headley never did get back to Arizona.

To go back a bit: this fellow Lewis was another of my mistakes. He seemed to have been the second least objectionable of the old SCS regional soils men and my first impression of him was good. He turned out to be a procrastinator. Of the mail that came in the office I needed to refer the bulk of it to staff members. Yet I wanted to avoid the red tape of special file slips. I simply had a note put on indication what was to be done. If a reply was to be prepared the note indicated for whose signature. One day we received a letter from a state conservationist asking

when he was going to get a reply to his memorandum of such and such a date. We looked for it and found it buried in Lewis' incoming basket. We talked about this at some length in my office to make clear that important mail should be answered within 24 hours, or, if that was not possible, acknowledge it and a tentative reply date indicated. In a few weeks the same thing happened again with the procedure explained more firmly. The third time it happened he was told that one more time would result in an unsatisfactory rating and a change in his job. Within two or three weeks he transferred to AID. This probably raised the average of both outfits.

I took the evening train from Lincoln to Chicago September 23rd and spent the 24th ^{writing} in a day room at the Morrison Hotel and left for Washington that evening.

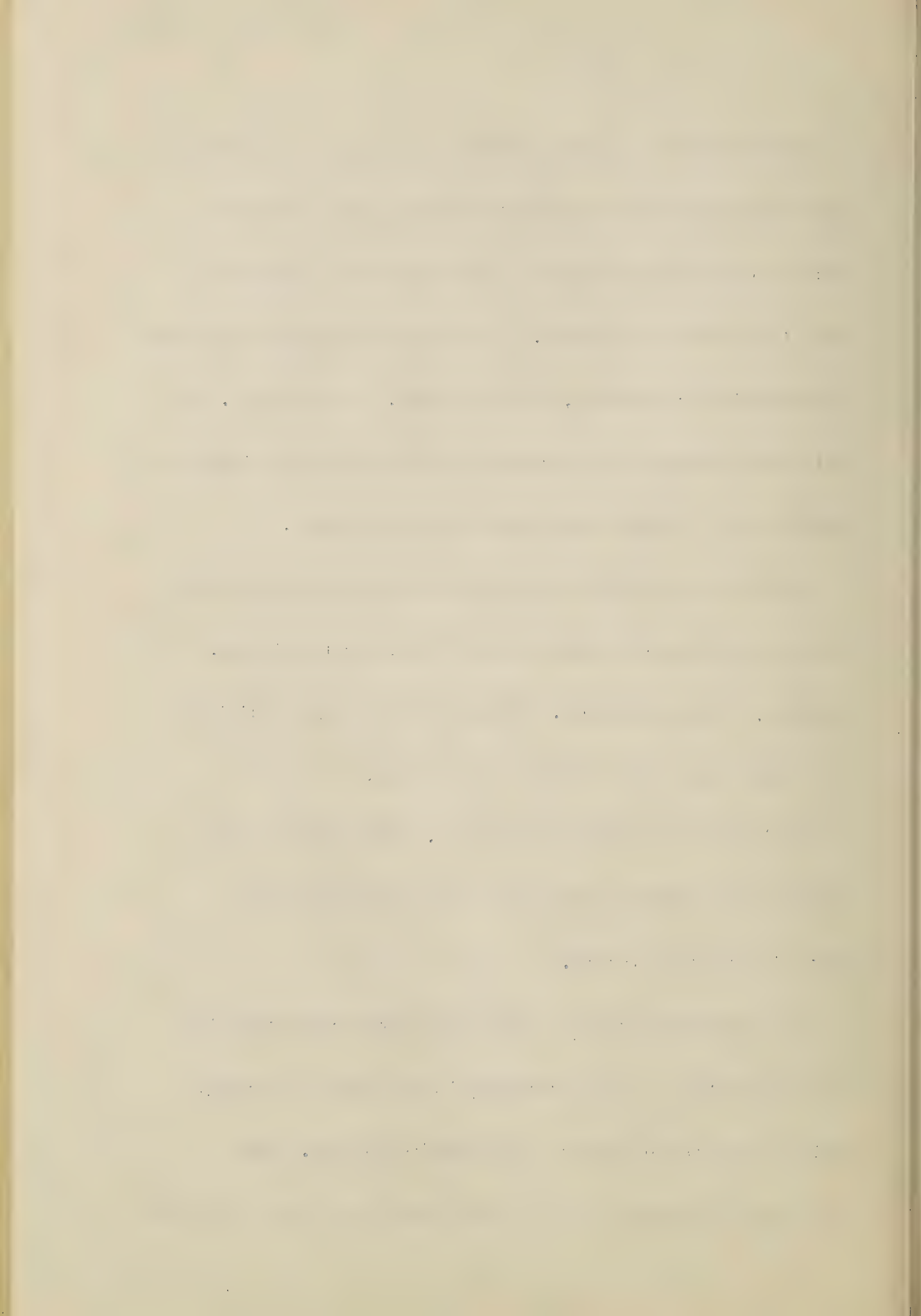
Much of October was spent on the Yearb^{oo}lk except for a trip to New Brunswick to give a lecture at the dedication of an old piece of woodland bought by the Carpenter's Union and to be used for research. My lecture was on Soil research in natural areas.

Beginning about the 20th of October we had nearly continuous sessions of the Yearbook Committee for three days. The members reacted to the annotated outline I had prepared with the help of some of the Washington members. We firmed up this outline and agreed on descriptions and authors, with alternates, for each paper. The whole was mimeographed as a prospectus and we got out the invitation letters during the next two or three weeks, as I recall.

Again we, in the Soil Survey, had considerable trouble getting the personnel people to handle some of the critical positions, especially in the laboratory. Personnel had so many "made" jobs that their regular work was hopelessly delayed.

During the first week in November Dr. Simonson met with the Canadians for their National Soil Survey Conference at the University of Saskatchewan.

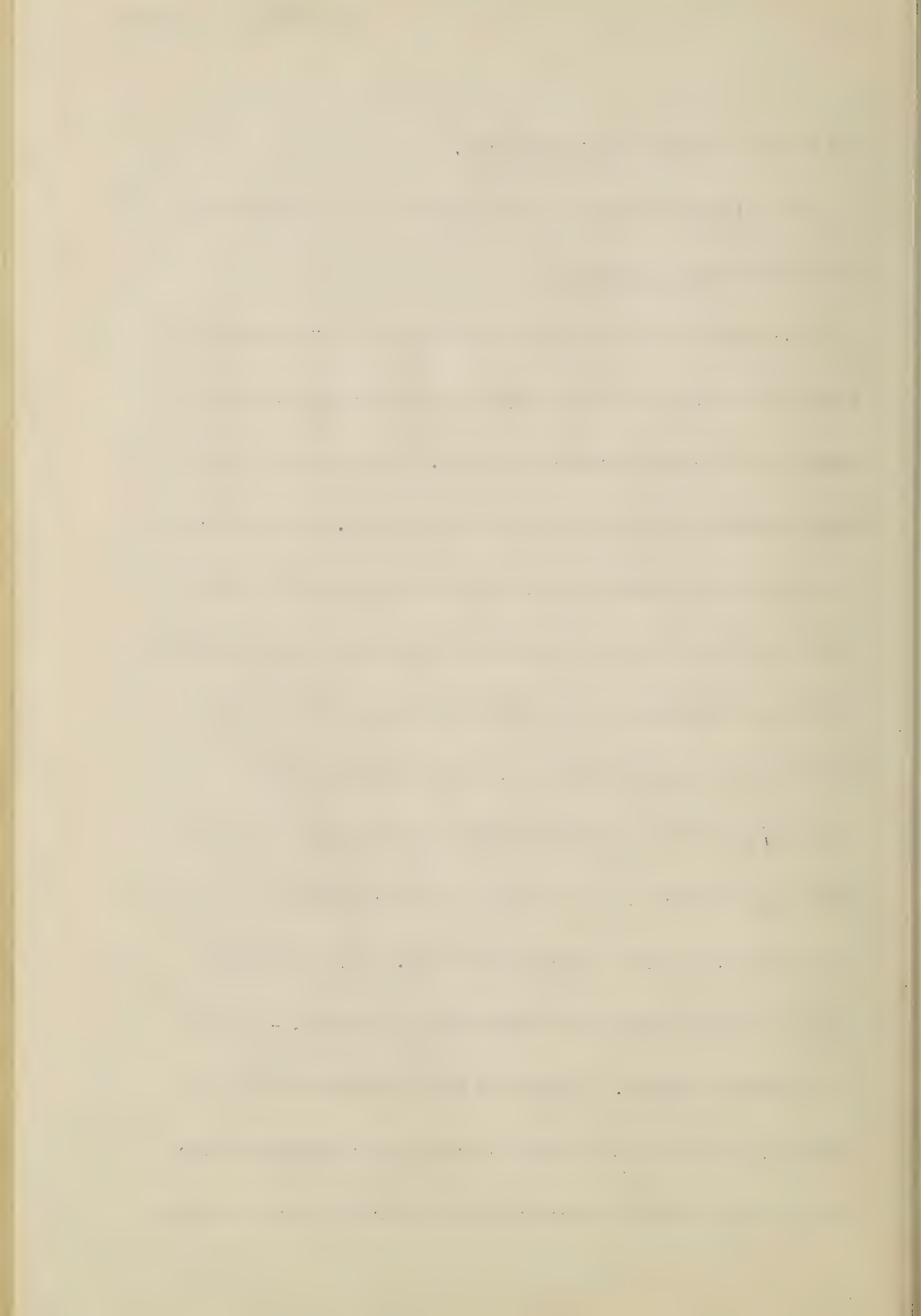
During November 2 and 3 I met with the state conservationists of the Cornbelt to go over procedures in handling soil surveys, including the role of various staff members involved. They had a good many questions but certainly their knowledge of the work



had improved tremendously in two years.

On the 10th of November I gave a lecture to the Capitol City Garden Club on Our garden soils.

From time to time I had had conferences with Cecil Wadleigh about how to get his soil and water research properly organized, especially the field management research. He was having a good many arguments with the state experiment stations. On November 10 I sent him a long memorandum, at his request, laying out a plan for a joint study by four outstanding state college people and four of his own scientists. This advisory committee would first be briefed on the responsibilities of the Department and then given an opportunity to study representative examples of work under way so that he could have advice on the respective roles of his group and the state experiment stations. If he did the job well he would have four good defenders of his program, - one in each land-grant region. I explained that the committee would be advisory. He would not be bound to accept their recommendations; still he would need to be prepared to consider them very carefully.



He thanked me for the memorandum and did nothing about it.

Relations with the experiment stations continued to deteriorate.

Wadleigh seemed to be afraid of everyone, even of me. I

happened to be in Iowa a few days after he had visited some of his

men on the campus there. He never even called on Dr. Pierre, the

head of the department where they were housed. Bill found out

about it just before I came and his irritation was not concealed.

Several of his men were even more discourteous. What a pity!

Yet Wadleigh invited himself to every national and regional meeting

he could work in of the NASCD. He was far more concerned about money

than with ideas.

November 13 and 19 were spent at Augustana College in Rock Island,

Illinois, attending a symposium financed by the Quad-City Institute

of World Affairs. Here I gave a lecture on Soils and agricultural

abundance. At this meeting, I met Mr. Lester Kellogg, an economist

with the John Deere Company, who impressed me very much. On

November 21 I had a leisurely trip back by train and I wrote a good

deal on one of the papers for the Yearbook.

On the evening of November 26 I took the train to Lincoln and sat through the early part of a conference among the soil correlators and state soil scientists of the Great Plains. On the evening of the 28th I went by bus to Manhattan, Kansas to take part in a land-use forum sponsored by Kansas State College. Here I gave a lecture on Relationship of soil characteristics to land use. (This was reworked a bit and published also in a local magazine as Our soil.)

On November 30 Arthur Nelson, the state soil scientist in Kansas, drove me back to Lincoln. This turned out to be a cloudy day. About half way to Lincoln Nelson made a wrong turning. I asked him two or three times whether he was sure of the road because it didn't seem right to me, but he was sure - until Manhattan came in sight again! After I left the Lincoln Conference the boys fixed up a special award and gift for unusual service. The gift was a compass. Despite his usual sense of humor, Nelson was not amused.

I spent November 30 and December 1 and most of December 2 talking over our many problems with the men at Lincoln and then went on home by train.

From letters sent to me at home and in other ways I began to sense a great deal of discontent in the Service, especially among the soil conservationists and the plant specialists. The general complaint was about the overemphasis on records and forms.

Throughout the year I had a heavy ^{correspondence} at home about the work especially with Retzer, Johnson, Baur, and Aandahl.

In December I started making plans for Lucille and me to go to Europe the next summer, especially for the Congress of the International Society of Soil Science in Paris. Major Ignatieff insisted that I spend ^d a week with him at Rome to make a final review of the manuscript for the second edition of the Efficient Use of Fertilizer. In a way this was unnecessary. But Ignatieff was very conscientious about his work and had no one of equal competence to consult. So I agreed to go much more to ease his mind than to prevent errors in the book.

After the usual holidays Lucille went with me to New York where I attended the annual meeting of the American Economic Association.

Part of the program was cooperative with the Farm Economic Association.

I presented a paper on World food and agricultural potentialities, which, curiously^M was published in the journals of both societies.

My paper went over very well. The room was completely full and many were standing. For us the highlight of the meeting was the presendential address of Dr. John D. Black. He fully deserved the great honor and attention he received from his fellow economists.

Lucille and I had a little time for shopping and returned home the evening of December 30.

1956. Although I had begun working on the Civil Service position descriptions for soil scientists progress was very slow.

Right after New Years we had upsetting news from the Gold Coast. C. F. Charter was taken seriously ill and was being flown to

a London hospital. *He soon died.*
After seeing him on January 4. I had had to tell him that his work was unsatisfactory. But it was very easy for him to get a high paying job in DCA.
 That year I had few formal speeches scheduled (although a great

many informal ones from outlines). Most of my available time was budgeted for the Yearbook. As he read papers Stefferud indicated words that he thought should be in the glossary in addition to the ones I had on a preliminary list. Many had been defined for the 1933 Yearbook and many were included in other glossaries. Yet a large part of these were not up-to-date or were not explained satisfactorily for the lay readers. I worked up a preliminary glossary and received criticisms from a few people close at hand. As the year went on the glossary was refined, mimeographed, and sent to all committee members and authors. As I recall, we had about three rounds of these drafts.

I attended the regional Soil Survey Work-Planning Conference in Raleigh, North Carolina January 10, 11, and 12. In attendance were

all the state soil scientists and soil correlators of the ^{soil survey} Service and representatives of each experiment station of the southern land-grant region. The meeting pleased me very much indeed. In 1952 and 1953 these were the most backward areas in the program. Glen Fuller had been shipped out. I had remarked earlier that we would know when we were free of the old bitterness: As long as we attended meetings where all the men were courteous and proper, we had trouble; but when we went ^{to} ~~at~~ meetings where the boys had their coats off, their ^{ties} loosened, and were arguing strenuously about soils, the troubles would be over. This was the situation at that meeting in Raleigh. An outsider couldn't have told who had been in the old Soil Survey and who in the old SCS. He would have known that Billy Ligon was the natural leader. It was a great pity that Hub Allaway became discouraged so early. In the committees one would find old SCS men arguing for more soil series and less phases! What a change! I spent quite a bit of time on the podium outlining our plans with special emphasis on training, reading, writing, soil survey interpretations, and the importance of soil survey work both within and outside the

Department. The boys had lots of questions and that was good too.

Near the end of January we learned of the death of C. F. Charter.

I wrote, as tactfully as I knew how, to Tom Mead and the Governor-General of the Gold Coast, and to G. W. Nye of the Colonial Office in London, pleading that full consideration be given to H. Brammer to take his place. This was done. But after independence, when the Gold Coast became Ghana, the pressure of natives against the Europeans greatly increased. In addition I suspect that Brammer lacked a bit in administrative acumen to handle these people, especially a little stinker by the name of Obeng. Finally Brammer quit about 1960 or 1961 in desperation and took a job with FAO. It was a great pity because Charter had built such a fine and useful program. Yet most of the results were published and will be available for sane people.

Saturday, February 4 I arrived in Boston and spent most of the day with Dr. Black's group at Harvard. Sunday Mary Alice and I had a fancy dinner and went to a movie. That evening and the next day I attended the conference of the National Association of Soil Conservation District Supervisors and took the late train to Washington.

The next week, while I was in the office, Roy Hockensmith had a heart attack. I rode in the ambulance with him and staid until his wife came. I had called her and told her to come but only in a taxi. It turned out not to be real serious and he recovered in a few weeks.

I spent February 8 and 9 at a state-wide meeting of the soil scientists in Georgia. The program had been fairly well arranged and several of the boys gave talks. It was perfectly clear that this group had a long way to go. I spoke to them for nearly an hour on the outlook for our work and for the Soil Survey as an organization.

Then in the evening I talked to the boys on the world food problem with emphasis on the humid tropics and an explanation of the bush fallow.

After my return from this conference I had a curious personal letter from one of the young men in the group.

Altany, Georgia
February 15, 1956

Dr. Charles E. Kellogg, Head
Soil Survey Division
Soil Conservation Service

Albany, Georgia
February 15, 1956

Dr. Charles E. Kellogg, Head
Soil Survey Division
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Dear Dr. Kellogg;

I had wanted very much to express my pleasure at meeting you at the training session in Athens. I meant to do this personally. Yet after your address to the group on Thursday afternoon there was no opportunity for it. Thus I am writing for that purpose.

I can truly say that I attended the meeting with feelings akin to speculation. You see sir, you have a reputation which is wide; yet not all of it is flattery. Also of one thing I am sure - you are aware of this.

So, to me one of the singular pleasures of the meeting was meeting and in becoming acquainted with you. It is too seldom that I meet individuals of renown and even of those, too many are satiated with feelings of self-importance. Those feelings were conspicuously absent in you.

I heartily enjoyed the occasions you spoke to us. Although you singled me out afterward on Wednesday as the one sitting in back and scowling, it was not in displeasure. The remarks you made to us were stimulating to thought. But with all the wisdom which is yours it is also very evident that you are a grand fellow. So, in closing permit me again to say, it was indeed a warm pleasure seeing you on that occasion.

Respectfully

Hubert J. Byrd
Soil Scientist

After the Soil Survey meeting Cecil Chapman, the state conservationist, and I went to call on the Dean of Agriculture and the Director of Extension at the University of Georgia. Undersecretary Morse had asked several of us to use such occasions for talking

to the college leadership about the Rural Development Program.

So far nothing had been done in Georgia but they did a good job of buttering me up and promised to have a state council meeting the following week. We discussed the selection of pilot counties for a while, and I returned to Washington.

I think that Undersecretary Morse was genuinely sincere in wanting to do something constructive for low-income farmers. He had appointed a committee, of which I was a member, but had no real organization of the work. If individual states or counties had some aggressive leadership something was done. We in the SCS had some money to expand our work in the pilot counties and so did other agencies. Specific rules were developed by a committee as a basis for releasing funds. But then the Undersecretary would approve certain counties or areas even though the work wasn't set up with local committees as he had demanded. With a few conspicuous exceptions, the program didn't get much off the ground.

About this time I had a dispute with the Undersecretary. He had finally seen a prospectus for our Yearbook on soil management. He

told the people in Information that the book was altogether too big and that it should be only a pamphlet. So I had to go to see him. He sat there pasty white from the want of being out doors at all. He was surrounded by piles and piles of manuscripts and docketts, the significance of many of which were beyond him. I really felt sorry for the man; he was so far over his head.

He told me that farmers wouldn't read a book. In his experience, he explained, the most popular reading in farm homes were small booklets. He went on some length about this.

Finally I broke in and said, "Mr. Morse, I also have been in many farm homes. I am not being facetious, but the two most popular books are the Bible and the Sears Roebuck catalogue. They are both large and no one has suggested that either be condensed."

Then I went on to explain the purpose of the Yearbook. We intended it as a manual for farmers and for those who work with farmers including county agents and work-unit conservationists. Few farm people would have occasion to read it as a whole. I took

as an example fertility, and listed the plant nutrients that were limiting factors somewhere in American farming. I said, "Let us take zinc for example. It is not enough to tell a reader that zinc deficiency may be a cause of low yields. We need to specify the more sensitive crops - pecans, citrus, corn, and soya beans. We should describe the symptoms.. Then we must explain how the deficiency is corrected. The method varies with the crop and with the soil. And so it is with all of the many nutrients plants take from the soil. Phosphorus deficiency is very widespread in the United States. Kinds of fertilizer to use depend on the soil. We need to explain the timing and placing of fertilizers. If we don't tell this minimum there is not much use of telling them anything." Finally the old man gave in and I have never been sure whether I convinced him or wore him out. But we got our numbers of pages for the Yearbook.

During 1956 highly significant cooperation was initiated with the Forest Service. Relations of the Forest Service had always been excellent with the old Soil Survey but poor with the SCS. Two originally independent events initiated this cooperation. John Metzger

had worked in the Soil Survey and was loaned to the Forest Service for the guayule work at the beginning of the war. When this was over the loan continued and he went with the Rocky Mountain Forest Experiment Station. He started to make a soil survey of a large experimental watershed with informal assistance on soil correlation from Arvid Cline and Dr. Aandahl. This job was far along before I knew about it. When the boys had finished it they asked me what to do to get it printed. I recommended that they explain what they had done in a joint memorandum to me and to Dr. McArdle, Chief of the Forest Service, and to request guidance. Mac called me on the phone as I knew he would and asked me how I intended to reply. I said, "If it is all right with you I shall recommend publication in the regular Soil Survey series of the Department. You know, Mac, we went through 17 years^e of having two soil surveys in the Department and I hope we don't need to do that again."

On this we were agreed and Retzer prepared the report in the usual way.

Long before this question arose I had begun to receive letters from various field officials of the Forest Service who wanted soil surveys made or who proposed various schemes. Some of these were good and others had been tried and found ineffective. From time to time I talked with some of the assistant chiefs but they knew nothing about these ideas.

Finally I went in to see McArdle, whom I knew well, - in fact he was clearly the outstanding Bureau Chief of the Department - and explained the whole matter. I said to him, "We should like to help your people but we don't know how to go about it. We don't know whether these people are willing to finance the work or not, and none of your assistant chiefs in Washington knows anything about it."

"Well," Mac asked, "what do you want me to do about it?"

"I wish you would get the best soils man you can find who knows your organization or can learn about it and put him here in your Washington office," I replied. "If that were done I would have my people look to that one man on all these questions of soil surveys in the National Forests. We cannot possibly know enough about

who's who in the Forest Service to work it out ourselves."

McArdle agreed that this was a good idea and he would take it up at a full staff conference that included the regional foresters.

Some two or three or four months passed and McArdle asked for a conference. He said, "The staff conference bought that idea we discussed on soil surveys in the National Forest. I didn't want to go ahead until regional foresters had fully committed themselves to use some of their operations funds. Now about this soil scientist for our staff here: Do you think John Retzer could do the job?"

"I'll answer that, Mac," I replied, "if you will answer a question for me first."

"What's that?" he asked.

"That neither you nor any of your assistant chiefs will ever forget that you asked me. You will recall that no names were discussed between us."

"Yes," he said, "I recall that you did not make a suggestion for an individual. We appreciate that and we'll remember it."

So I told him that John Retzer was the best man I could think of for the job. Yet I wasn't too sure that he could be enticed into an office job. If he did come it was very important that he should not be called a "Kellogg appointment."

All this lead to a formal memorandum of understanding between SCS and the Forest Service on soil surveys.

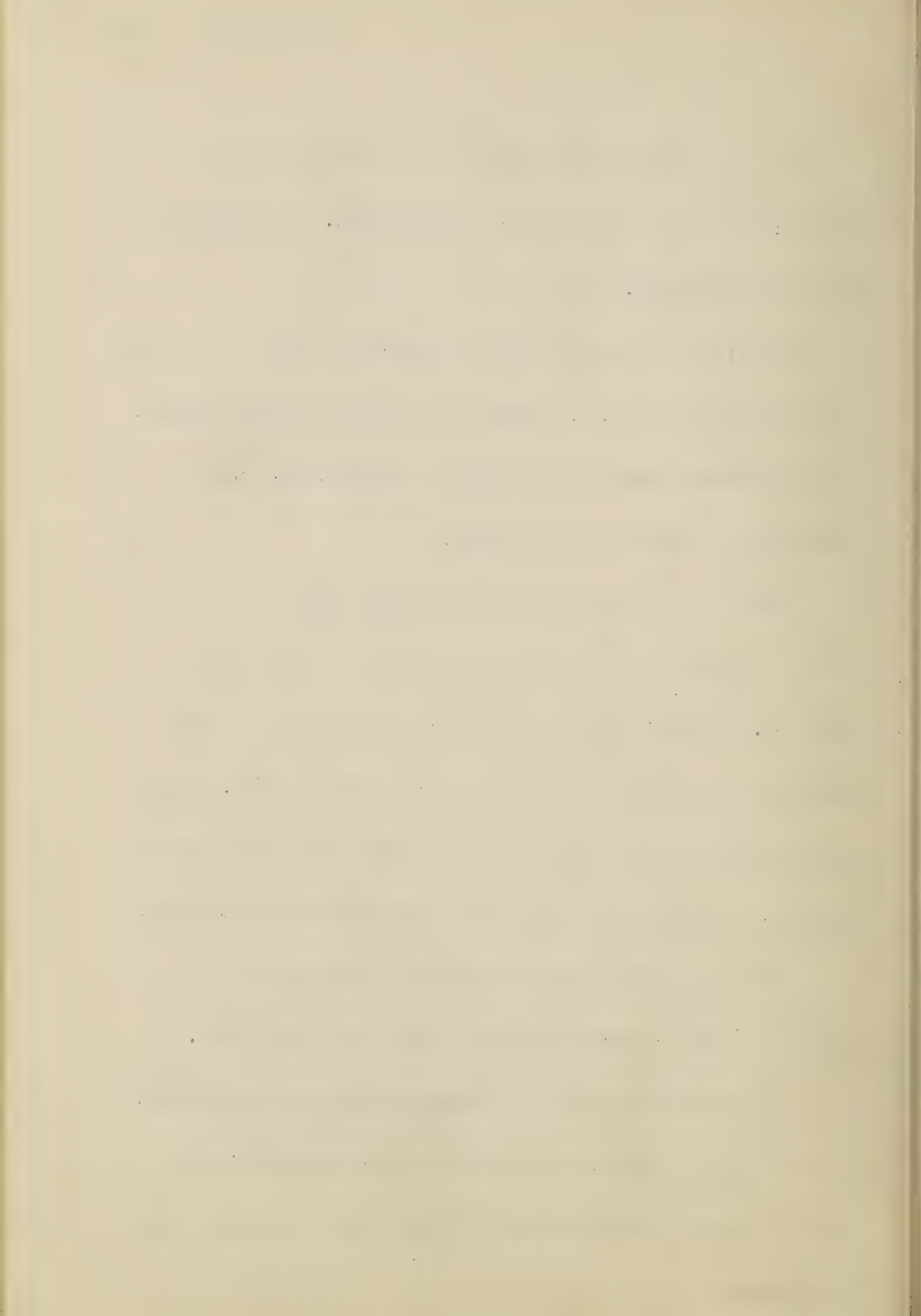
The SCS haters in the Forest Service griped and so did the Forest Service haters in the SCS. Even Hollis Williams and a few others said that this memorandum was simply the entering wedge for the Forest Service to take over the Soil Survey! I told such critics, "If you find a policy or a person in the Forest Service handicapping our work I shall try to work with the Forest Service for a change. I might not succeed but we have gotten along well before. Of course we cannot deal with vague generalities. But understand this: If I find anybody in SCS or in the Forest Service trying to sabotage this program, which is so clearly in the public interest, on bureaucratic grounds I may not be able to do them any harm. But believe me it wont be for want of trying."

Of course a lot of arrangements had to be made and policies adjusted; and as time goes on others will be needed. The program has been highly successful. *up to about 1969.*

March 19 to 23 I made the program review of SCS work in Louisiana with the assistance of L. V. Comptom, A. C. Watson, and Louis Herndon. Watson and Herndon went with the deputy to different areas than Compton and I visited with H. B. Martin.

The first day I had an amusing experience. They had chosen to take me to Bossier Parish where they had a standard soil survey under way. I asked Martin what the district board thought of this and he had to admit that he had never discussed it with them! (This turned out to be a good lesson for him and since then he has been very active in this area with the local soil conservation district boards.)

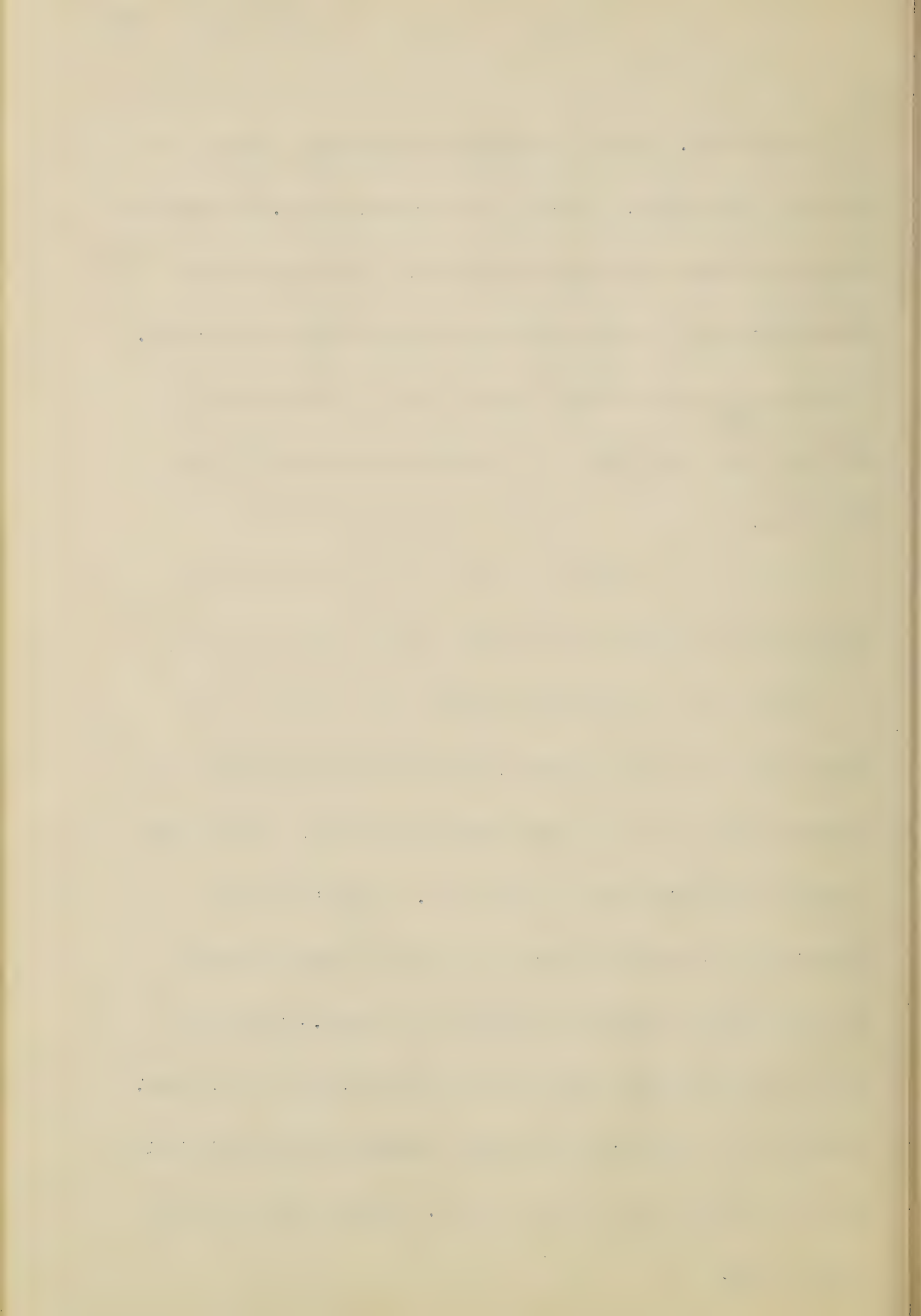
There were several difficult problems in Louisiana but I thought the state conservationist had quite a good view of them. I was not much impressed with D. L. Fontenot, the state soil scientist. He knew quite a bit about soils but was most too argumentative to work with people. (In years ahead this got so bad that he had to be transferred.)



Although Mr. Lytle of the agricultural experiment station is an excellent party leader, I received the impression that somehow relations with the SCS state office were not too good. Lytle was quiet and Fontenot was rough and argumentative and this may explain the trouble. As we met in the state office it became clear that many phases of the engineering work needed to be strengthened and plans were made to do that.

This was a very interesting visit for me because many of the problems were different from those I had normally been dealing with.

After completing the program review H. B. Martin took me to Baton Rouge to interview the Dean of Agriculture and the Director of Extension Service about the Rural Development Program. Obviously the Director of Extension wanted no part of it. He didn't like the emphasis on low-income farms and had no plans for calling a meeting of agency heads as Undersecretary Morse had directed. He said that such low-income problems as they had were being solved by other means! It was true that industry was expanding remarkably along the Mississippi River between Baton Rouge and New Orleans. Yet not enough jobs were being created.



Director Sanders kept insisting on the "need for legislation".

He said he knew the Undersecretary wouldn't like it, but that's the way things were in Louisiana. He thought it would be difficult to add to what they were now doing. Most towns had committees to try to bring in industry. The state already had a large program for vocational and trade schools.

During this unsatisfactory conversation Dr. J. N. Efferson, Director of the Experiment Station, listened without saying anything. He observed that I was taking notes. As we started to leave he asked me to come into his office a moment. He told me that the old Dean, who was away, didn't know what it was all about and wouldn't do anything.~~xxxxxxxxxxxx~~ He said, "I can't do anything now so I didn't say anything. I don't agree at all with Sanders. Beginning next July 1 I shall be Dean. Then we shall cooperate as the Undersecretary desires."

"This means then, " I said, "that I should prepare two memoranda for Mr. Morse, one summarizing Sanders' views and a confidential one explaining your position come July 1."

"That is correct", he replied.

So we took our leave and I went on down to New Orleans and got the plane for Washington.

My correspondence now was heavy with plans for the Paris Congress and FAO matters. Both Professor Tavernier and Dr. Ignatieff were having trouble with Luis Bramao. (This was only a slight foretaste of what was to come.)

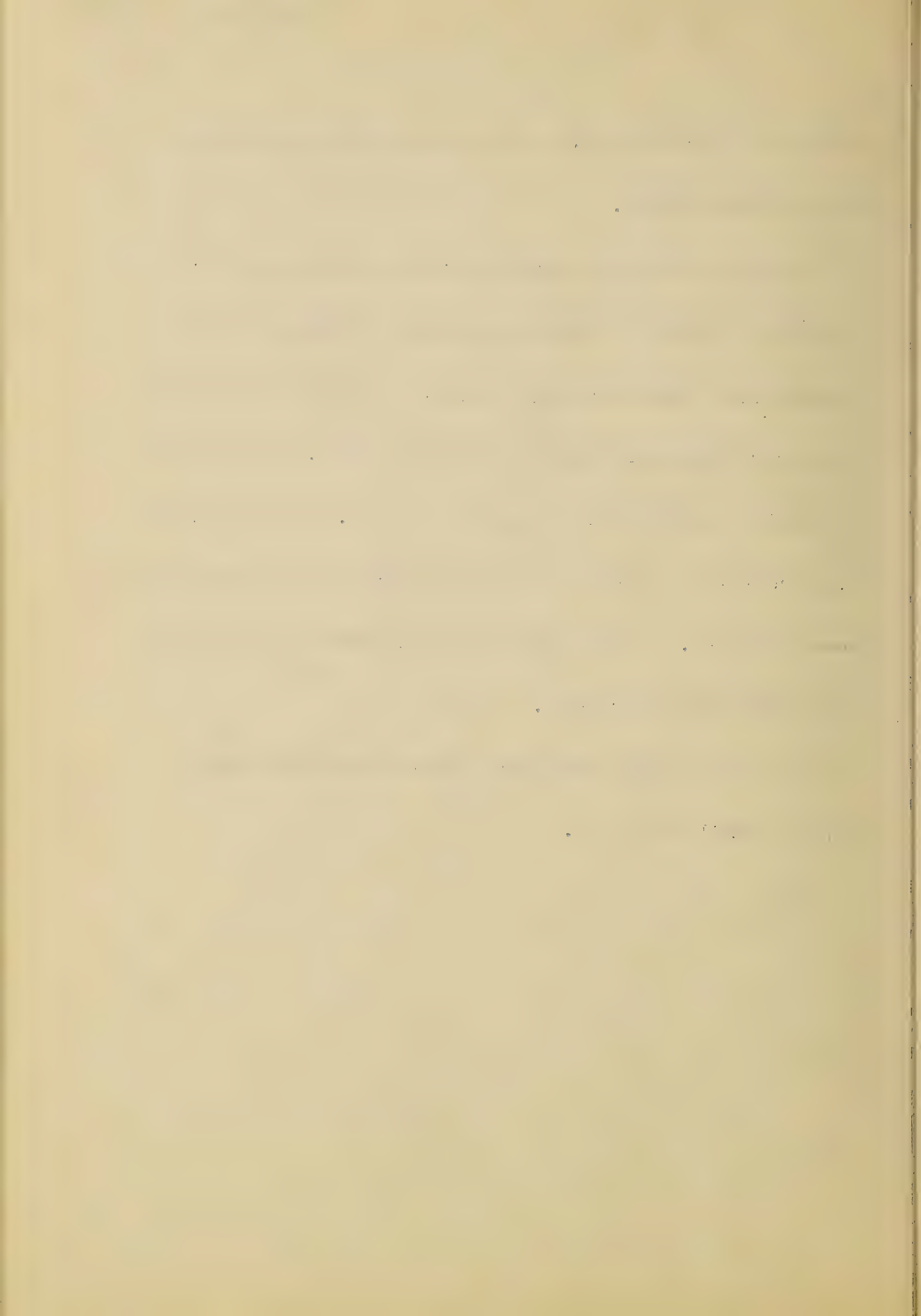
On April 4, Dr. John McClelland was appointed as senior correlator with Mr. Johnson. We had had a real struggle over this. As a Canadian citizen he had gone to Iowa State for his graduate training and then took a position with North Dakota State. He had agreed to come with us over a year earlier. Before he could do that his final citizenship papers had to be signed by the judge of the federal district court. Just before that was to be done the judge left. Then Eisenhower and the North Dakota senators rowed for nearly a year about a successor. Hence the delay. *But for an excellent soil scientist.*

I continued to have heavy correspondence with the principal soil correlators, especially with Johnson and Aandahl, about what

to do in the backward states and about the new cooperative program with the Forest Service.

That month Robert had permission to ride with the Air Force to Iceland. Actually the boys doing this went to McGuire Air Force Base and took a chance on getting a plane. He waited for a while and then took a commercial flight on an Icelandic airline. Men in British and American uniforms were not popular in Iceland. So he changed to civilian clothes in the air station and our friend Bjorn Johannesson looked after him. He had a good visit at the University and did get a ride home with the Air Force.

We had the National Soil Survey Work-Planning Conference in St. Louis April 30 to May 5.



April 30 to May 5, 1956

Again I reviewed some of our principal problems with the group. As usual most of the work was done in the committees after brief review of the ^{four} regional work-planning conferences.

Dr. Smith was making progress with the new system of soil classification. He now had a Fifth Approximation in progress. At this conference he opened up the question of nomenclature with the help of Dr. Heller, Professor of Classics at the University of Illinois. We were hoping to have names for the groups that would be connotative. Each one needed to be a noun that could easily be made into an adjective. Since we would use many ^{roots from} Latin and Greek and ^{some from} many other ^{languages, which} roots that would go together in different combinations, ^{we} had to be careful that we would ^{not} come up accidentally with vulgarisms in some other languages.

Dr. Dawson was making further progress with the classification of organic soils that he explained to us.

That year the horizon nomenclature had been turned over to Dr. Marlin G. Cline who could at least discuss the problems without getting the people angry as Whiteside had done.

At this conference we had a committee on engineering applications and worked up tentative plans for having a chapter on soils engineering in each published soil survey.

We had good discussions of the place of slope in soil classification and further improvement ⁱⁿ the classification and mapping of eroded soils. Both of these came back to the Manual ^P We had good discussions on several less important subjects but the greatest difficulty came over Mr. Klingebiel's attempt to explain the so-called capability classification.

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It was decided to have a group of people to make lists of

names we would use in the future and make sure that they would

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of the future of the soil classification system.

Waller's attempt to explain the so-called capacity classification.

He had made a lot of general statements in his presentation that were inconsistent with one another and the boys gave him a bit of a hard time. I realized that something really radical was going to have to be done to get rid of these inconsistencies.

After the conference was over Paul H. Montgomery and I left on the same train. As I recall, he got off around midnight or perhaps sometime after that. Anyway, we sat together in my roomette until nearly midnight talking about this difficult problem. How much of it came from Montgomery and how much from myself I could not honestly recall. Yet when I went to bed that night I knew what had to be done and roughly how it had to be done. We would have to involve all the ~~State~~ soil scientists/ in such a way that they would think they had a part in developing the idea. ~~and~~ I also decided that night that I wanted Paul Montgomery on my staff in Washington. (Later, this was arranged ~~for~~.)

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a way that they would think they had a part in everything.

I also realized that night that I wanted to get Montanary and my staff

Washington. (Later, this was arranged for.)

For some silly reason the meeting of the state conservationists for the Northeast was held in a little semi-resort village in Pennsylvania called Potato City. As I recall two or three of us from Washington went at the same time. Visitors were met at the Harrisburg airport and SCS men drove us in government cars to this place. So many other people were there from state and field offices that one could hardly find the state conservationists. The program was poorly arranged. Afterward I complained about this as a colossal waste of funds and effort.

The Yearbook was coming along well although I had to waste a lot of time pacifying the authors to whom Stefferud had written his characteristic letters. Unhappily, I didn't have time to read all of the papers. Later I discovered some serious errors due to the authors or to Stefferud, or, more likely, to bad communications between them.

It seemed necessary to visit South Carolina. The state office wanted to promote some boys that were questionable and I wanted to see them. Also there were some bad failures of farm planning around Charleston. Governor Herter had bought an old place and spent a lot of money in draining and other improvements. The first year he got

a prize for the best pasture in South Carolina. About three years later the soil was essentially unproductive.

I went to the state office at Columbia and several of us took a field trip together. One of the most interesting items was the practical drainage of forest land to improve its production. The land already had a harvestable stand of the slow-growing pond pine. They laid out a combination of roads to get to the pine and ditches that were dug for drainage and to get earth to make the grades for the roads. Once drained, transplanted seedlings of the normal species of pine grew well.

I was able to visit a famous old plantation near Charleston on the margin of a tidal channel. The long entrance road was bordered by two rows on each side of beautiful live oaks with great stringers of Spanish moss. Part of the movie, "Gone with the wind" had been filmed here. Then one went by the old slave quarters before coming to the, "big house".

A comparative young couple were buying this place - I gathered rather on a shoestring. The man was a lawyer in Charleston and his

wife looked after the plantation. I met her at the "big house" and spoke about her azaleas. She asked me to look at them because they weren't doing well. It seemed obvious that they were getting a bit of salt water. She was pleased to know the cause, because she had another source of water and then took me through the big house. This was really a lovely place.

We then looked at a vegetable farm near the tidal area. As I recall, the owner was an old-time district supervisor and president of the State Association of Soil Conservation Districts. Our people had built two ponds for him for irrigation water. Both were too close to the salt water for the sandy soil and his tomatoes had all the characteristic symbols of excess salts. At one stage the owner called me to one side and said, "I don't want to pry into the organization of the Service but I do have a question: I notice that the professors in the college here (The Citadel) and the physicians in the hospitals go away occasionally for advance training and refresher courses. I've worked with the boys in the Service here for years and none of

them has been away. How do you know that they are up to date in everything?

This was about as hard a question to answer as I ever had because these boys had made a mess of his farm. (As soon as I could, which took a lot of arranging, I got Alexander down there and we organized a special training school in Charleston on salinity in soils and cat clays.) Later, an oil company bought this man's land as a site for storage. So the SCS mistakes there were covered up!

We also looked at some interesting experiments in a wildlife refuge to find the best combinations of fresh and salt water managed with dikes and canals.

The next day we looked at some of the rice fields where Governor Hester's farm had failed. The drainage had brought about the formation of very acid clay loam, what the Dutch called "cat Clay". Later I got the people from the laboratory to sample it and the pH was about 3 or a little more. The farm planner had sent a sample to Clemson and received word that it had a lime requirement of two tons to the acre. Obviously something had gone wrong since it would be about

50 tons. Later I read some diaries of some of the old rice growers around Charleston. They had learned empirically to keep the soil wet all the time except for the brief interval during harvest. Most fields were fallowed every other year. The hurricanes brought in the sea water. In the presence of organic matter, the sulphur in it was reduced. Nothing particular happened so long as the soil staid wet; but once drained sulphuric acid formed and made the soil exceedingly

acid. Reclamation costs, at that time at least, would have been too high to be practicable.

TP We were very pleased that Mr. Lyle F. Alexander received the USOA Distinguished Service Award (Gold Medal) this May.

A little after noon May 19 Tom Buie, the state conservationist,

and I left the boys and started for Tampa, Florida to attend a meeting of the state conservationists of the South, which began on the 21st. As usual in southern meetings I sensed a lot of bureaucratic politicing and speculation.

I talked to the group about our soil survey program with special emphasis on training and organization within the states. I also led a discussion on the Rural Development Program. At that point plans

June

called for about three pilot counties or areas per state.

Also at this meeting there were too many people. At least half of those there opened neither their mouths or their notebooks during the meetings. Later I complained to Mr. Williams about this and we got the numbers cut down a bit. At least he agreed to not more than one soil scientist from each of the principal soil correlator's office.

June 4 to 8 I made the program review of Service work in Illinois with the help of J. J. Coyle and Wallace L. Anderson. On the whole I was very much pleased with the staff and the work going on in Illinois. We did have to argue with state conservationist B. B. Clark a bit about his failure to carry out the Service memoranda on biology. We told him we could not order him to do it nor could we release him from the obligation. Only the administrator could do that. So he agreed to carry out the policy.

Bartelli was doing an excellent job of the soil survey program under less than ideal conditions. The soil scientists in the experiment station did not want to change any of their old ways and insisted

on publishing all of the surveys. But they hadn't the money to do it.

So we had a problem to solve before long. Except for that, they had an excellent program in Illinois.

I attended a state meeting of Service personnel in Louisville, Kentucky on the 13th and 14th of June. They gave me quite a work out at this meeting with one lecture to the soil scientists, one to the soil conservationists, and one to the whole staff. The sectional programs were fairly good but the general program was not well planned.

I spent two days, June 25th and 26th, with the cartographic heads at their annual meeting in Milwaukee. These men made a fine group.

On July 6 my status was changed to GS-16 the first step in the super grades. I learned that Assistant Secretary Peterson had to look over my old FBI file. One of the agents had told me some of the things in there. One informant said he had seen me reading a copy of the "Daily Worker." When the appointment was cleared I spoke to Mr. Peterson about his unhappy experience of reading this file. He said, "There's nothing in there you need to worry about." This was all right for him to say but one never knows how someone might interpret such irrelevant information.

In July we finally got agreement on a plan for each principal correlator to ^{have} have a two-week training school in soil correlation. Each state could have on the average between one, or possibly two, candidates. Fortunately all of the principal soil correlators had had some teaching experience. So many of the in-service training programs of the government were only a little more than vacations that I impressed on the men the importance of making these good solid weeks with examinations. These sessions proved to be extraordinarily valuable and resulted in much better work and stimulated several of the men to undertake further study. So in July we sent out a schedule for the following winter and asked each state conservationist to make one recommendation with an alternate.

As I recall now the Yearbook was pretty well put to bed in July.

On August 5 Lucille and I left for Europe. (The details of this journey are set forth in the Western European Journal, 1956.) This was to be partly work and partly holiday.

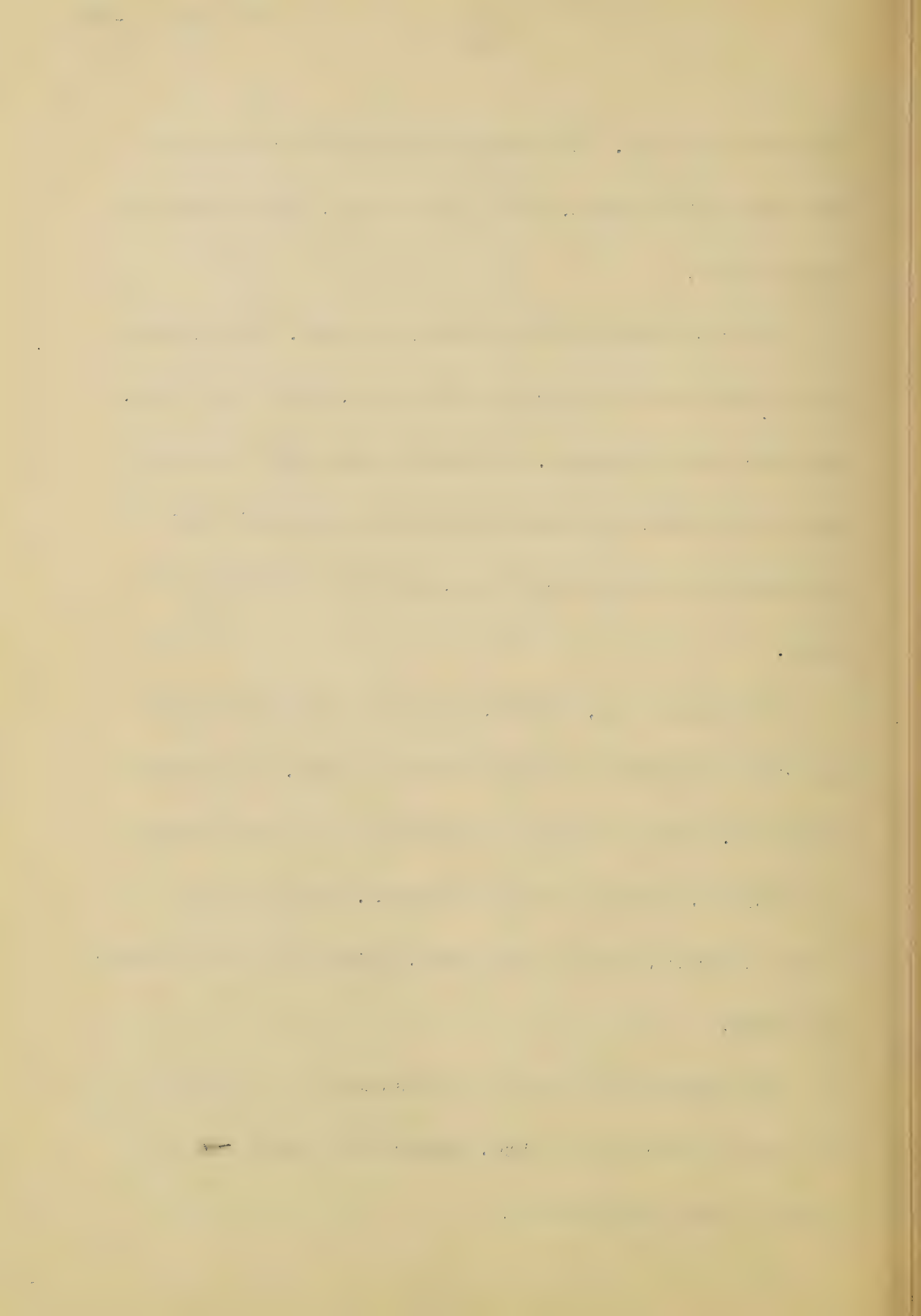
We arrived in Paris a bit before noon August 6, at which point I started annual leave. This was the first holiday we had taken

together in some time. We picked up our tickets for the journey to Rome from American Express, called at the Embassy, and had dinner with Georges Aubert.

On August 7 we took the early train to Avignon. The following day we hired a car and driver and visited Arles, Nîmes, Pont du Gard, and other points of interest. The unusually cold ^rspring had killed many of the olives. The French had gotten well started on their improvement plan for the Rhone Valley, which was to take several years.

The following day, August 9, we went in the other direction and visited Carpentras and nearby villages and farms. We returned about noon. After we had paid for the car and got our key I became seriously ill, apparently from food poisoning. Armed with her sturdy (?) French, which was good enough, Lucille got me some paregoric and kaolin.

The next morning I was able to dress and get to the train and we went on to Genoa for the night. We were glad then to have the American Express people meet us.



By the next morning, August 11 I was all right and we went on to Florence and had that day and two more in the loveliest of European cities.

We went on to Rome August 14 and staid with the Ignatieffs. The Major and I had nearly a continuous session on the second edition of The efficient use of fertilizer. (And it paid my expenses - Paris back to Paris!) The Major and I are both early risers so we started out with tea on the terrace about 6:00 a.m.

Lucille had more time for sightseeing in Rome, which does not appeal to me much except for "Moses" and the Sistine Chapel. She also took a three-day tour of Naples, Pompeii, Sorrento, Capri and the Amalfi Drive.

Besides the conferences with Ignatieff I had meetings with Whalen, Blackmore, Schickle, and others in FAO. Whalen spoke to me at length about the loose morals in FAO, beginning with the Director General's Office, especially Mr. Dodd.

Ignatieff had arranged for me to give a long seminar in FAO on the bush fallow and shifting cultivation. For some curious reason

FAO essentially ignored this important system of tropical farming except for a few highly irresponsible, emotional statements by the ~~for~~esters.

We spent August 23 and 24 in Venice, then went on to Geneva, and finally came to Paris for the 6th Congress of the International Society of Soil Science the evening of August 28.

Most of my old friends from Europe were there and we had a lot of good talk. For the first time since World War II, the Soviets sent a delegation. They were very friendly and brought me gifts of vodka and caviar.

As the days went on I learned from Lyle Alexander, Guy Smith, and others of a sort of conspiracy promoted by Tyner of Illinois and Hans Jenny of California to prevent me, of the USDA, from being named as President of the 1960 Congress to be held in the United States. Except for Ableiter's brief period, Tyner followed me at North Dakota. He had little success. Somehow he disliked me because of his failures.

Since about 1952 the State Department ceased appointing official

delegates to international meetings of which the government was not a party. As the international representative for the Society I called a meeting of the delegation, in Paris, which included all Americans present. Most of the Americans at the Congress were from the colleges and, on the whole, they did not include a majority of the top men in the Society.

I had seriously erred in not having the matter settled of who was to be nominated President by the Soil Science Society at its previous annual meeting.

The upshot was that Bradfield was nominated as the result of this intrigue, although I doubt that he, himself, knew about it. I had worked so hard to get the International Society going after the war that Tavernier, Edelman, Aubert, and my other European friends were amazed. But they understood this USDA-land-grant politics when it was explained because they have their own brands.

While in Paris I talked with Edelman about cat clays in the United States. We discussed my recent observations in South Carolina.

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I told him that I wished he might come and look at them. He replied that he would be glad to come. We discussed dates and I told him I would look into the finances as soon as I got back.

He said, "You needn't worry about that, I won't need any money from you."

I asked him, "Did this new wife bring you a lot of money or what? How can you make trips like that without reimbursement?"

"You don't understand," he said, "how appreciative my government is for the enormous help given us by your army engineers in Europe after the big storms. They brought in whole battalions of engineers stationed in Europe. Besides all the help we even learned something from you. Our government would be glad to find some favor they could do in partial return. All you need to do is to send me an invitation to help with this problem."

So it was arranged and later he and a young Dutch engineer, Van Staveren, made a long visit in the field.

Since I had not planned to take the Congress tours in France, Lucille and I flew to Brussels September 9 and went over to Ghent.

I spent a day with Tavernier at his Institute and on a little trip to Brugge where Lucille bought lace for Mary Alice' bridal veil that would ~~never~~ be needed in 1957.

The next morning we made an extra package of some light clothing and other things that Tavernier agreed to send by boat mail. As he gave the directions to his secretary in Flemish, I said, "I just don't believe you can communicate in that language." (It turned out they couldn't. He also gave her a package of things to send by air to the Congo. Our unnecessary stuff went by air and the things for the Belgian scientists went by boat!)

In the late morning we drove to Brussels and had lunch with Dr. Henri, Director Of IRSIA. Then I visited Jurion. In the evening we had dinner at their home.

The next day, September 12, I spent at Louvain with Jurion, Fripiat, Laudelout and Mlle. Gastuche. Lucille spent the day with Mme. Jurion. I had a heavy day at Louvain and we covered a great deal of ground.

Again we ^{had} ~~have~~ dinner chez Jurion.

The next morning Jurion and I visited Professor Von Strahlen at his home and Professor Homes at the University of Brussels.

We took the plane just before lunch and were down in London at 1:45. Here we did a little sightseeing and I spent most of two days at Rothamsted.

The evening of September 16 we left London for New York. We reached Baltimore about 11:00 a.m. and had lunch in the airport while Robert drove over for us.

Juris was had reported August 30. to be with us for a year in the position Pete Lewis had left.

About a week later Robert went to Iceland for the academic

year at the University in Reykjavik. He had been awarded both a Scandinavian-American Fellowship and an Icelandic (Government) Fellowship. Joan didn't go with him then but gave up her job at the National Institutes of Health in mid-November, spent a little time in Boston, and went to Iceland in early December.

From September 24 through 28 we had the annual meeting of state conservationists in Washington. As usual I went over the Soil Survey plans and problems with each of the five groups of men with special

emphasis on the position of the soil specialist within the state, training, interpretations, and research. The greatest value of these meetings to me was the individual conferences with the state conservationists about specific problems in their states.

The previous summer the Congress had approved the Great Plains conservation program to be administered by the SCS in counties designated by the Secretary within the Great Plains area as defined in the legislation. Under this program individual farmers and ranchers could receive both technical assistance and cost sharing under a contract based on a farm plan for efficient sustained production. Plans for this program took a good deal of the time of the conference.

We had a position set up for an Assistant Director of Soil Survey Interpretations to work with Klingbeil^{ie}. He was quite strong for Patton, the state soil scientist in West Virginia. I hardly knew him. Since Morris Austin was going there on a field review I went along for three days in the field with them. One evening while

relaxing after dinner Patton spoke of his Sunday School, which he seemed to enjoy. A few Sundays earlier an economist had talked with them but he didn't enjoy it. He said, "You just can't make any sense out of what an economist says." So then I knew he wouldn't do.

I then talked to Klingebiel about Paul Montgomery and he said he would be very happy with him. We had Headley on the staff from the West and all the rest of us were mid-westerners. We needed somebody from the cotton country. Paul was not deeply trained in theoretical soil science but I had been much impressed with his judgment in the field and on the so-called capability classification. So I wrote Montgomery a long letter and asked him to come. He replied that he would rather stay where he was. I felt sure that he was simply afraid of the competition among all these mid-westerners. Most southern soil scientists have a feeling of inferiority about their home universities. Even Joe Moon had the feeling! I wrote a long, confidential letter to Dr. Buie, knowing that he would show it to Montgomery, explaining why I thought Montgomery hesitated and that this

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was the wrong kind of reason for both him and the Service. A week or so later Montgomery agreed to come. This strengthened the staff significantly.

Early in November I wrote Don Williams a long memorandum on the critical recruitment problem in soil correlation. Although he never came to recognize fully the scientific training required for success in this work, by hard pushing we finally got GS-14 for the principal soil correlators, nearly a year later. But our big difficulty with the status of the senior soil correlators remained. For many years this problem remained unsolved.

The meetings of the Soil Science Society were in Cincinnati November 12 to 16. At the urging of the plant technologists of the Service I went a few days early and spent Friday and Saturday with them. This was a mixed group left over from the old regional offices. The leadership was very poor and their jobs had never been well defined.

I attended many of the scientific meetings but spent much of my time in individual conferences about the cooperative work and on the plans for the Seventh Congress to be held in the United States in 1960.

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In the middle of December for three days I attended a general meeting of soil scientists in Ohio. They seemed to be having continued trouble getting along among the agencies. The big difficulty was poor leadership beginning with the state conservationist, Tom Kennard. He had urged me to come to this meeting. He was at his office all the time but never came in even to greet the men. Instead he sent his deputy, named Alger, who was even more stupid and made a very poor impression on the men. They did have one big red-headed young man, Mike Stout, who stood out over the others. Not many weeks later I was able to suggest him to another state for a promotion.

Mary Alice came home for Christmas⁵. Robert and Joan were in Iceland.

One of my last jobs of the year was to write a statement for Byron Shaw to use with the Secretary and Undersecretary on why it was necessary to have a National Agricultural Research Center! The reasons were so obvious that it seemed a pity such memos even had to be written.

1957.

January. For some time ~~now~~ ^{had} I have been working on a book on soil conservation. Although several "scare" books have been published no positive and constructive book on the subject was yet available. With the great demands on my time in the Soil Survey and with the problems of agricultural development in the less developed countries this project may need to be postponed or perhaps abandoned entirely. ~~I~~ ^{yet might} I will have some good source material though for the future. (Not finished & until much later)

Early in the year I had a letter from Opleng from Ghana. He was ~~is~~ a little stinker with influence in high places and wants to replace Brammer, the successor of Charter. I ~~try~~ ^{had} to reason with him about the great need ~~for~~ ^{for} developing good scientists among the young people of Ghana so that they will be ready for senior positions at some future time. But to sacrifice the basic scholarship requirements and leadership now could easily result in such poor work that the government would no longer finance the Soil and Land-Use Survey. He continued, however, to make trouble and finally Brammer had to leave about 1961 for work with FAO in East Pakistan.

Arrangements were made this month for Dr. Smith to have a few weeks in Australia and New Zealand.

While with FAO in Rome last summer I arranged for Jack Wetzel to take part in a watershed protection conference and training session in India. I thought he could do a good job and I think he did. Yet it turned out that he was so inexperienced in foreign travel that he was full of complaints at the end about hotel and travel accommodations in India where most people travel without difficulty.

On January 10, after much preparation, I arranged a conference with D. A. Williams about the critical situation respecting our small but excellent field soil correlation staff. Several of the men were being offered better positions and competent men were exceedingly scarce. From the conference I mistakenly gathered that he understood the situation. He gave me a clear verbal approval to go ahead with allocations of the principal soil correlators from GS-13 to GS-14 and of the senior soil correlators from GS-12 to GS-13. These changes were highly necessary. (Yet a year later he reversed himself on the senior soil correlators, which resulted in the greatest blow the Soil Survey was to have for the next ten years. I don't know whether he was being "coy" with me at the time or whether Mohagen talked him out of it, or just what forces were involved.

Some of the staff felt that Roy Hockensmith was in the act to help some of his old friends. I do not know. It became clear later that I had made a very serious error in organizing the soil correlators in the 1940s for economy. Rather than have all the senior correlators officed with the principal correlators, I had arranged free space for them in the land-grant universities. This reduced cost for rent and travel. It gave each of them access to a good library and strengthened our federal-state cooperation. Yet in the SCS this left one man in a few states that did not report to the state conservationist. It turned out that the few thousands of dollars that we saved may have cost us several hundred thousand dollars in efficiency from lowered morale and inadequate guidance.

On Sunday I left for Monticello, Illinois to take part in a Midwest regional conference of the National Cooperative Soil Survey.

A good deal of the time of this conference was spent, and mostly wasted, in trying to figure out rather bizarre and elaborate symbols for the designations of soil horizons in accordance with their genesis. Actually, of course, firm decisions on fine points cannot be made in the field along^c without laboratory determinations. Further, a man can write a few words of description of what he sees that are very useful without going beyond what he is reasonably certain about. A large number of such bizarre symbols were proposed for B horizons.

During January 17 and 18 I made the long train trip to Boston. To while away the time I poked fun at the boys with a little poem -- A Lament for B. When I returned home Mommy typed it and I sent the boys a copy. (Insert 695a, 695b, and 695c).

January 19 our daughter, Mary Alice was married to John Schaefer at the Church of the Redeemer in Chestnut Hill, Brookline, Massachusetts. Mr. and Mrs. Matthew Roberts very kindly invited us to have the reception in their recently purchased large home in Boston. It was a lovely wedding.

During the next few days I had very heavy office work, which included more difficulties with Stefferud over the 1957 Yearbook. It was too bad about Stefferud. He was a good craftsman as editor but he managed to get nearly everyone angry and resentful. This made lots of trouble for the chairman. For example every document without exception about this Yearbook gave its title as Soil Management. Except for the briefest possible statements of some basic properties of soil, and relationships among them, the whole book dealt with soil management. Yet at the last moment, without any consultation,

Now it seems to me
That busy as a B
Finds its manifestation
In the Fifth Approximation
Is it B or not to be
Betwixt the A and C?
Or has it lost its old position
For new-found erudition?

Once I knew one B,
As simple as could be;
She wooed me with her true location,
Not clay-skin deep solicitation.

A big and strong old B
A B two could be.
When flashy charms defied notation
I glorified the soil description.

Was she a Mother B,
Full of many a little B?
For with today's atomic fission
The fate of B is long division.

A dozen B's for B
Wont stay in A to C.
With a B for each attraction
They'll compete for habitation.

Without old Mother B
Who'll say what is a B?
Who'll see they have protection
From geological distraction?

The death of Mother B
Marks the end of royalty.
How can we maintain distinction
Midst so much discrimination?

B's loss of royalty
Means not democracy
For plain men will give distinction
To the obvious field condition.

Thus how give nobility
And keep reality A and B
With all the B's in true relation
And not one lost from indecision?

And not one lost from induction?
With all the B's in true relation
And keep reality

To the obvious field condition.
For plain men will give distinction
Means not democracy
B's loss of royalty

Might so much discrimination?
How can we maintain distinction
Marks the end of royalty.
The death of Mother B

From geological distraction?
Who'll see they have protection
Who'll say what is a B?
Without old Mother B

They'll compete for habitation.
With a B for each attraction
Wont stay in A to C.
A dozen B's for B

The fate of B is long division.
For with today's atomic fission
Full of many a little B?
Was she a Mother B.

I glorified the soil description.
When flashy charms defied notation
A B two could be.
A big and strong old B

Not clay-skin deep solicitation.
She wooed me with her true location,
As simple as could be;
Once I knew one B.

For new-found attraction?
Or has it lost its old position
Betwixt the A and Q?
Is it B or not to be

In the Fifth Approximation
Finds its position
That busy as a B
Now it seems to me

Can we take the lab to see
All soil morphology?
Or do we add to field condition
Genetic truth from revelation?

Let's call the roll to see
What subtle galaxy
B's new algebra of relation
Displays in Fifth Approximation:

(I'll be easier for me
To list the kinds of B
Without continual alteration
In the metric composition.)

The old podzolic texture B
Has clay skins now to make it be,
And if there are no skins to see
It still may be a clayey B;
Or better still, try hard to see.
We now abhor transitional B:
Can it be that it is no B?
I'll leave it in my list to see
If structural B and color B
And latosolic B will be
Enough to keep horizon C
Down where I think it ought to be.
Next we come to a flirty B
With a red-brown dress that I can see.
But is it humus that I see?
Or is it iron and is it free?
Or alumina, or all three?
And not mixed with a clayey B?
Well fares the solonchic B
If caps are clear for all to see;
But not so clear the gleyic B,
And still less so the limy B
Unless it's well above the C.
Now what becomes of old B three?
An evil rumor came to me
That it could even cease to be,
Portioned out to a new C B
And to an elevated C
That's being pushed up by a D
The Manual says should never be.
This evil thing to poor B three
Emerges from geology.
Weathering now belongs to C,
Soil formation to A and B;
But if we fail to keep them free
We'll have no more pedology.
Only regolith will still be
And that can hardly have a B.
If strong men scorn such heresy
We'll always have an A, B, C.

Can we take the job to see
All soil morphology?
Or do we want to find something
Gentle from the landscape?

Let's call the roll to see
What subtle galaxy
B's new algebra of relation
Displays in Fifth Approximation:

(I'll be easier for me
To list the kinds of B
With a transitional situation
In the middle composition)

The old podzolic texture B
Has clay skins now to make it be,
And if there are no skins to see
It still may be a clayey B;
Or better still, try hard to see.
We now savor transitional B:
Can it be that it is no B?
I'll leave it in my list to see
If structural B and color B
And latosolic B will be
Enough to keep horizon C
Down where I think it ought to be.
Next we come to a flinty B
With a red-brown dress that I can see.
But is it humus that I see?
Or is it iron and is it free?
Or alumina, or all three?
And not mixed with a clayey B?
Well takes the solonchak B
If caps are clear for all to see;
But not clear the clayey B,
And still less so the limy B.
Unless it's well above the C.
Now what becomes of old B three?
An evil rumor came to me
That it could not cease to be.
Portioned out to a new C B
And to an elevated C
That's being pushed up by a D
The Manual says should never be.
This evil rumor to hear is true
Weathering now belongs to C,
Soil formation to A and B;
But if we fail to keep them free
We'll have no more pedology.
Only regolith will still be
And that can hardly have a B.
If strong men scorn such heresy
We'll always have an A, B, C.

We all have seen a buried B;
That is not rock, because it's B,
Even though we call it C
To soil above that has a B.
Anthropic A can be Ap,
But so may too anthropic B.
On B two m none yet agree
Since fragipan has gotten free.
Then too we had a u on B
To show an unconformity;
But now the purists say to me:
"You mean, discontinuity".
Our list has outgrown brevity
So I'll but mention fractured B,
Broken by the Manual D
Or cut in two or even three
By tongues of A two shaped like V.
Of course, we have eroded B
And "topsoil" made of it and C.
But let us keep old subsoil free
And add more lines for each new B.
(See page one hundred eighty three
On how to subdivide a B.)
I failed to list I see
The most important B,
Because the Manual has notation
Where we lack an explanation.
It provides for one, two, three;
So now we need another B.
We'll need a B for hesitation -
A B horizon of frustration.
One sigh for Mother B's
And in her name a plea
Don't lose her major connotation
By unneeded mutilation.

(Later published)
By Bill Jaimson

We all have seen a buried B;
That is not rock, because it's B,
Even though we call it C
To soil above that has a B.
Anthropic A can be A,
But so may too anthropic B.
On B two m none yet agree
Since trispan has gotten free.
Then too we had a n on B
To show an anthropic B;
But now the buried B is not
"A" and "B" are not
The last one is not
So I'll not mention trispan B,
Problem of the human B
Or not in two or even three
Ly tongues of A two shaped like V.
Of course, we have eroded B
and "anthropic" B of it and C.
The last one is not
And add more lines for each new B.
(See page one hundred eighty three
On how to subdivide a B.)

I tried to let I see
The most recent B,
Because the human B is not
Where we lack an explanation.

It provides for one, two, three;
So now we need another B.
We'll need a B for hesitation -
A B portion of frustration.

One sign for Mother B
And in her name a plus
We'll have her better explanation
By repeated explanation.

(Handwritten)

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he changed the title to Soil. This title was very misleading since the book dealt only with soil management. A great many people were confused. They supposed this Yearbook to be a revision of the 1938 Yearbook, which was a larger book and did deal with the whole field of soil science. When the 1957 book came out I was somewhat disappointed. I had been able to make a close technical review of nearly all of the papers for the 1938 book but not very many of those in the 1957 volume. Several rather bad mistakes got by, although few were critical for the ordinary reader.

Early in February I started a long trip in the west. First I stopped in Denver and our people there in the State Office took me to Brighton, a small town serving irrigation farmers and some ranchers. I gave a speech to the farmers there on The principles of soil management. I think it went fairly well although the acoustics were poor and the speaker stood a long way from his audience. The address was followed by a rather poor question period. At the conclusion of the session a reporter from the "Denver Post" asked me for a copy of my speech, which I gave him. Later he wrote a story for his paper that quoted me as ~~being~~ opposed to Secretary Benson's crop control program! No aspect of this program was mentioned in any way. What a reporter!

I left that evening for Portland, Oregon and had a visit with our people in the cartographic unit the next day. The following evening I went on to Berkeley.

On February 7 I had a meeting with the soil correlators and state soil scientists that went very well. The next day I had a good conference in the state office including Dean Aldrich of the University. This helped to clear up some of our problems of cooperation, but not completely. One never does in California.

The following day, Saturday, February 9, Bill Johnson took me on a little field trip in San Mateo County. Previously I had commented on a soil correlation because of several mapping units that were listed as "Class VIII". I raised the question of whether these separations were valid and necessary. When Bill proposed the field trip he mentioned my interest in how far the sea salt had an influence on soil morphology. This question still bothers me. In New Zealand the effect is noticeable clearly three or four miles from the sea and in Western Australia scores of miles. We drove to the sea and found very little effect beyond a few hundred yards. Then Bill suggested we drive back a different way, through the hills. He stopped at a heather farm. Mediterranean ~~Heather~~ was cut and sold to florists' shops all over the country. I saw some in a nice New York City apartment only a few weeks later. The soil requirements of this plant are quite specific. The manager showed us the whole complex operation. Then we continued on with brief stops at other special farms producing exotic flowers or other plants. After the last stop Bill then explained that each of these kinds of farms was on a different kind of soil and all of them were in Class VIII in respect to field crops. This was a good demonstration in practical psychology. Obviously, I withdrew my objections to his correlation.

That evening and the next two days I spent on the train home which gave me time to catch up on my writing for the Americana and on speeches scheduled ahead.

After two days at the office I went to Cornell for an address to the local Soil Conservation Society Chapter on Soil management goals and soil conservation. I don't think I ever had a speech that went over so well. Both the audience and I had a lot of fun, especially

in the question period. Dr. Brady was pleased no end.

Just as I left on this trip someone handed me a book on Soil Conservation by J. H. Stallings. I read it on the train and checked parts of it when I returned. Stallings had had the job of "summarizing" articles in this field to send out to the staff. Many of these reviews were quoted. He obviously didn't understand them. Whole sections of the Soil Survey Manual had been copied with scarcely a word changed but with neither reference nor acknowledgment! While he was copying and writing the book he had gotten angry with me and with my secretary because we wouldn't give him an advanced copy of the glossary I had prepared for the 1957 Yearbook.

On February 20 I was in New York City for a meeting of the American Pulpwood Association. Many of the members were becoming interested in using our soil surveys for planning their operations including land purchase. I gave them an address on How soil management aids growth, which was published in their Journal. This talk went over extremely well and we had an excellent question period.

I still did a lot of writing on the conservation book that will probably never see the light of day.

Near the end of February I began work with an inter-agency committee for the promotion of wide-scale translations of Soviet scientific literature. The fact is that much of it had been translated and some of this ^{was} ready for publication. This program grew enormously during the following years, especially with blocked currency from Public Law 480. Foster Mohrhardt did a lot to promote it. Clifford Orvedal helped on the soils part. Many standard text books were

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published and cover-to-cover translations of many Soviet scientific journals were initiated, including Pochvovedenie. (I was unhappy about the title chosen for this. It had always been called pedology in English but for some strange reason the non-descriptive title Soviet Soil Science was chosen.)

On March 5 Mr. D. A. Williams told me that he wanted to raise the grades of the state soil scientists to GS-13 and leave the grades of the senior soil correlators at GS-12 and have the senior correlators report to the state conservationists. And to make matters worse he changed this to have them report to the state soil scientists! Only 2 or 3 of the state soil scientists in the country were as able as all but 2 of the senior correlators. As I have commented before this move was destined to cost us tremendously in the quality and amount of work. The only possible partial solution was to pick the best ones for assistant principal correlators and start out to develop some young men interested in science to have a correlator in each state. Obviously this would take a long time and we could expect to lose several good men in the process, which we did and to have much more unpublishable soil survey work than necessary.

On top of other criticisms, had been one that "people don't use the soil surveys." I insisted that these charges were false. We had many satisfied users. Still the claims were made, so a statistical study was agreed on.

Early in March I had a request from the National Broadcasting Company in New York to work with one of their men on a live TV show

on the origins of technology and the differences in the way it spread in Western Europe and America as contrasted with what is now the Soviet Union.

The week of March 11 we had in St. Louis the Work-Planning Conference of the National Cooperative Soil Survey. These meetings make good training sessions for our people and enormously helped the morale. Scholarly soil scientists are so few in the Service, and have so few peers to talk with, that they tend to get discouraged. But at these meetings they find that the others are having the same kinds of problems and their morale is lifted. *Insert 700a. 700b*

Immediately following the conference I went to the University of Arkansas to give a public lecture on Opportunities for improved soil use. This seemed to go well. I had conferences with the president and others. The president told me an interesting story about how the president of the University of Oklahoma almost lost his job. He was supporting his request for a new library before a reluctant committee of the State Legislature. Almost in desperation he said, "I'm trying to build a university that our football team can be proud of."

I pointed out to Professor D. A. Hinkle that Dr. L. T. Alexander would be an excellent candidate for a D. Sc. from Arkansas where he got his bachelor's degree. He was enthusiastic over the suggestion and later I sent him the material. In due course Alexander received the D. Sc.

I came back to a very heavy desk, ~~/~~ Along with quite a bit of work with Undersecretary Morse on a rural development program. He was a nice old fellow and meant well, but he was in over his head. On top of that the garden season began.

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1957

We again held the National Soil Survey Work-Planning Conference in St. Louis the week beginning March 11.

We dealt with many of the old problems because in the Soil Survey we have some changes in problems and many changes in the answers to the same problems. At the beginning this year I highlighted the great importance of pushing our publication of soil surveys. Because most of our users do not have the data available in any other way. At this time we had also considerable prospect of expansion of the survey program with the likelihood of needing more staff.

Although we had made progress in soil correlation, we had much further to go. Our need was for more precise correlations, better definitions, and a smaller number of mapping units, especially phases. Many of the phases put on a map were really not defined units, but notes that should have gone in the notebook instead of on the map.

I stressed the immediate necessity for improving our capability groupings. This was to occupy Mr. Klingebiel and Mr. Montgomery for some time. We also talked about the importance of more study within each soil survey area. A routine soil survey was not a good kind. Each area had its own potentialities and problems that should be explored and sorted out.

Several State conservationists and State soil scientists were using published soil maps in farm planning and there was no reason why others should not also.

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Several State conservationists and State soil scientists were

using published soil maps in farm planning and there was no reason

why others should not also.

Since Dr. Cline had taken over the Horizon Committee good progress was being made for improvement in terminology and descriptions that would be more helpful in connection with the new system of classification.

Dr. Dawson was making good progress with the classification of organic soils. Perhaps our greatest progress was in the field of engineering interpretations and in the improvement of a small-scale, general soil maps.

Although the many technical reports are valuable and useful, the main benefit from these meetings was their value for training.

It was being made for improvement in terminology and description that would be more helpful in connection with the new system of classification.

On April 2 I went to New York for the TV show, which was broadcast here early Sunday April 7. I received a nice check for this but it could have been a lot bigger had not the whole group taken me to a very expensive nightclub!

Friday, April 5 Margaret Drummond, the sister of Winnie Muir, came for a two-week visit. This had taken considerable preparation to be sure she wouldn't be a charge on the government of the United States! Our agricultural attaché in London, Dr. Englund, was very helpful.

Finally Dr. John Retzer joined the staff of the Forest Service to be in charge of their soil surveys and to work with us. Despite my heavy garden season I talked with him all day Sunday, April 7.

We had our House Hearings on appropriations April 12. So far these men don't realize the broad importance of the National Cooperative Soil Survey program outside the SCS.

Two days later I had an all-day meeting with Orsini and the heads of the cartographic units.

In Paris in 1956 I had talked with Professor Edelman about our problem of "cat clays" in the lower Coastal Plain and invited him to visit us the following year. He agreed to do that but he would want to bring along an engineer. I said, "I'll try to make arrangements for your travel." He explained that wouldn't be necessary because the Dutch Government felt so indebted to the United States for the

enormous help given them by the engineering units of the American Army in Europe after the big storm that ruined so many dykes in 1951. He said that they thought they knew all about engineering but found they didn't when the Americans moved in. It happened there was a big international conference in the West at which the engineer, Van Stavern, would attend. We here worked out a very detailed itinerary, beginning in Texas, just after the western conference. So Edelman came and spent the night with us, Sunday, April 21 and then went on the tour with Van Stavern.

The following week I had to make some budget cuts below the increase we had requested, which is never pleasant. In the SCS one never knows for sure how much support he has from the front offices all along the line.

About noon, Sunday, April 28 I left for Cambridge to take part in seminars at Harvard. This gave me a chance to see Mary Alice again and she had dinner with me at the home of Dr. Brinser. All day of the 29th I spent with Mrs. Black, Galbraith, ^{and} Brinser and with the graduate students. Brinser gave me a copy of his request for funds for the seminar made to Resources for the Future. He wanted me to criticize it. I read it over on the plane and found it completely unappealing. I doubted that he would make it at Harvard.

Near the end of this month I received a copy of a ridiculously critical memorandum of our procedures in the Soil Survey sent out by Francis Hole of Wisconsin to the State Experiment Station Soil Survey leaders of the Midwest. Obviously he had copied the suggestions of Eugene Whiteside of Michigan State denouncing committees for soil

correlations and other highly impracticable suggestions. This fellow Whiteside has a lot of ability but is desperately anxious for greater recognition. Like several others he tries to get it by destructive criticism and silly little suggestions rather than by hard work and study. I wrote personal letters to Frank Riecken and to Robert Muckenhirn. They were both quite disgusted, especially Muckenhirn, who took care of the situation quite nicely.

On May 1 Paul H. Montgomery joined the staff as Assistant Director for Soil Survey Interpretation. When I came into the SCS I knew we would have to do something about the "sacred cow" -- land capability. At a meeting in St. Louis Klingebiel had tried to explain it and got himself completely wound up in inconsistencies. (He later became a very good man after about 3 or 4 years of intensive training by me which took an enormous amount of my time.) Montgomery and I sat up on the train ^{coming} back from that meeting and discussed the question. When I went to bed that night I knew what needed to be done. It took a lot of effort to persuade him to take this position in Washington. But he proved to be extremely helpful. Since he was the only qualified soil scientist in South Carolina I knew his replacement would be difficult. Dr. Buie, the state conservationist and old regional director, was a very poor judge of men.

We had our Senate Hearings on the morning of Monday, May 6 and in the afternoon I left for Spartanburg, S.C. I spent some time with our people there and then went by car to Brunswick, Georgia. That evening I met Edelman, Van Stavern and a large group of our people on tour

with Edelman. Our Dutch friends demonstrated a special augur and apparatus for determining the permeability of wet soils. We examined many of the clay soils and related soils including some that had gone bad as a result of drainage. (I shall not include all of the details of this very interesting field trip since they are well documented elsewhere.) We made studies around Fleming, Georgia and between there and Charleston, South Carolina. During part of our excursion the leader of the soil conservation districts met with us. We looked at some interesting drainage for pulpwood. We found some laterite under a Ground-Water Podzol.

Edelman was having some trouble with his ulcers but we carried on.

On Sunday, May 12 I had a most interesting conference with a Mr. Samuel Gaillard Stoney. Although a bachelor, he lived in a nice little house by himself surrounded by relics of his French Huguenot family. When he spoke of "The War" he always meant the War Between the States. He was historian of Charleston and edited a ~~large~~ *local* journal of ^{or} genealogy -- a subject of vast importance to the local citizens. Almost immediately he pointed to an old assortment of silverplate on the mantle and said, "We buried that three times in our garden, once against the British, once against the Spanish, and once against the Yankees! None of them found it." As soon as I could I got the subject on the tidal gates. These were reputed to have been copied from the Dutch. I explained that the Dutch never honed their gates so these must have been copied from the French. This pleased him no end and he gave me many suggestions for reading and we exchanged reprints during the next month.

We continued the field trip north of Charleston, especially in the old rice fields and found a great deal of cat clay. I found also that our soil scientists there had done a poor job of soil description and that they knew next to nothing about geomorphology. As a result, some of the soils were badly classified. I left the party from Wilmington, North Carolina and returned to the office on May 21.

On Thursday, May 23 I attended an interesting session reviewing the history and use of the Soil Survey of Fairfax County, Virginia -- the earliest one made specifically for urban planning. (The history of soils engineering work is briefly summarized in the first chapter of a small book on Soil Surveys and Land-Use Planning published in 1966 by the Soil Science Society of America.)

On May 23 I awoke at 5:30 a.m. with a terrible pain -- just awful. Mommy took me to the Group Health Clinic and Dr. Rosenbaum said that it was a kidney stone. I asked for morphine. During the day I was between nightmares and awoke at short intervals. I saw clearly hundreds of soil profiles in New Zealand, Australia, Africa, and Europe, which I could not have called to mind normally without notes!

The next day I had another attack but demoral kept the pain down and I passed the stone. This left me quite weak and I would have stayed home longer had it not been for Edelman being here. I went down Friday afternoon to introduce him for a lecture in the Secretary's conference room. It was one of the best lectures I have ever heard. At the close I had to say: "Gentlemen you have all heard talk against the 'lecture system'. You have had a beautiful demonstration that it all depends on the lecturer."

Edelman left Saturday, June 1 and Tavernier came Sunday, June 2. For the next two days we had meetings with the principal soil correlators. Part of our discussion was about the strategy to be used in the special expansion of our Soil Survey in the Great Plains as directed by the Undersecretary.

Tavernier stayed with us and visited with the other men while I caught up on office work. About Friday I got an order to be prepared to go in a plane on Sunday, June 9 with Vice-President Nixon and party to Michigan State University where he was to deliver the commencement address. As a student I never expected to go in a car from the airport to the campus led by state police with their sirens on with no stops for traffic signals. Obviously I got a lot of kidding, even from my own family, because of my well-known lack of admiration for the Vice-President.

As time went on my back got to be very sore and my stomach a bit uneasy.

In June we initiated a study of grass-soil relationships in the area of Utah, eastern Nevada, southern Idaho, and western Wyoming and Colorado. Two excellent men were assigned to this research -- Vern K. Hugie and H. B. Passey -- with headquarters in Salt Lake City.

June 24 Tavernier returned to Belgium and I went to East Lansing to give an address at the national meeting of the agricultural engineers where I talked to them about soil classification, soil maps, and soils engineering.

While there I met Al Moseman. The Government of India wanted me to visit there and advise on soil survey and soil conservation. Rockefeller had agreed to finance the trip. Because the end of our

leave period came well into January, by leaving January 1 I could do the trip on annual leave and get my full salary twice. I tentatively agreed to the plan.

On June 23 I left the meetings and had a conference with Sackrider, our state conservationist, and Dr. Turk, director of the Michigan experiment station about our cooperative soil survey in Michigan. Whiteside had been making all sorts of suggestions. He wanted the federal publication limited wholly to the map and soil descriptions and then have the state university publish everything on interpretations. Through Jim Porter I had learned that the other members of the department didn't agree. More important this plan would ruin the Federal Soil Survey. If our published soil surveys had only soil maps and descriptions, appropriations would cease. I didn't get anywhere but I'm sure I shook Turk's confidence in Whiteside. We had a general meeting for lunch and I spent the entire afternoon with Whiteside and his associate and the SCS staff. Whiteside was downright insulting. I told him that we would publish the soil survey whether they did or not. I thought we had an agreement on a special organization to handle management classes versus capability classes but he didn't like that.

He called me very early the next morning and said he realized he'd been a bit silly and wanted to have breakfast with me. But then at breakfast he repeated the whole weary story. Oh God! Oh Montreal!

Engberg and Harmon came by with a car and took me to Detroit for my train. On the way we stopped to see Christmas tree production, which is more nearly norticulture than forestry. A couple of days after I got back I wrote a letter to Turk and discussed it fully

with the staff before hand. It was a courteous letter but between the lines it said that if Michigan couldn't cooperate SCS would have no choice but to go ahead alone. Later he replied exactly along the lines I had proposed earlier with copies to Whiteside and the others.

At lunch on June 24th Byron Shaw raised the question of whether or not I would take a position in FAO as Assistant Director General. I had forgotten for the moment about my schedule in Michigan. So later I called and said that if I had to make a decision then the answer would be, "No." Then later I was asked to reconsider and didn't. Apparently Orris Wells, Ralph Roberts, and Lick McArdle were urging this. But there was more to come.

On July 5 I got word that our appropriation wasn't what I had hoped but still not too bad. I had a lot of garden work to catch up and many conferences. Among these was one at Plant Industry Station with Dr. Bauer and all the state soil scientists of the Northeast. He is certainly doing a good job training these boys. He handles them a bit like a schoolmaster. In fact, he reminds me of a mathematics teacher who became principal of the high school.

On July 29 I was told, by Byron Shaw, as I recall, that I should plan to be asked to go to the Soviet Union as part of the cultural exchange program.

Since Ignatieff had invited us to their home at Richmond in Quebec, during the period he was on home leave from Rome, Monray and I decided to drive up there. We spent a bit over a week on the trip and there. It was a pleasant and restful interlude but my back and stomach continued to bother me. We were delighted with their daughter Mika and with a nephew.

While there I had a telephone call from Washington saying that the cultural exchange program with the Soviet Union was to be postponed.

The day I returned to the office, Joe Bulick of CIA came to see me. He had been telling me how anxious they were to get samples of handwriting for character reading. In fact, I had cooperated with them by getting a few people whom they were interested in to write something out for me. These notes I gave to Joe. A few weeks before he had asked me for certain others and I told him that I didn't have much confidence in this business. So I reached in my desk and pulled out one of my field notebooks. I cut out two leaves, four pages, on the middle of a field trip in New Hampshire. He agreed to get it analyzed with the analyst knowing only three things; that I was white, an American, and a male. He showed me the results and I was flabbergasted. He had about $2\frac{1}{2}$ typewritten pages of an extraordinarily accurate diagnosis. I don't see how they do it. He even emphasized what Carl Dorney, our budget officer, told me: "You are almost too careful about the use of government funds!"

We were now arranging for the 1960 Congress of the International Soil Science Society to be held in Madison, Wisconsin. Dr. Smith and I worked especially on the tours. We planned to have one before the Congress from New York City to Madison and two following the Congress -- one from Madison through the Middle West and the South terminating in Washington and the other from Madison through the Midwest and the Great Plains to Riverside, California and terminating in San Francisco. First of all we had to negotiate with the State Department about where we could go since Soviets would be along.

This turned out to be a very difficult job. Then we had a committee for each tour. They had to prepare tour guides that we needed to check. We had to arrange for a ~~man~~ ^{Special} man on each tour. Then arrangements had to be made for large pits in the soils. But all of this work paid off very well. Before the Congress we had three excellent published Tour Guides edited and printed by the Department of Agriculture. These represented an enormous amount of work by many people. And the results were greatly appreciated by our foreign visitors.

On top of everything else in August, I had to be acting administrator quite a bit with my back and stomach about killing me.

In August we began to have a great deal of trouble with soil surveys cooperative with the Bureau of Indian Affairs. Don Simpson, a former state soil scientist in Texas, was a poor man and had been encouraged to accept an offer to look after the soil surveys in the Bureau of Indian Affairs. Simpson hadn't learned anything or forgotten anything in ten years and was sore at the BCS. Thus for a long time we had trouble except in those states that had strong local men in the Bureau and ignored Simpson.

Then too, in August Dr. McCalab, one of our very best senior correlators, and also a good soil mineralogist, got an offer from an oil company and decided to leave. This was a hard blow indeed -- the fruit of the bureaucratic politics about the senior soil correlators. And I knew that we would lose most of the other good ones if I couldn't wangle better situations for them.

Along about this time I also had a good deal of correspondence

with David Gardner, then the state soil scientist in New York, about his doctor's thesis at Harvard which I was hoping could be published since it dealt responsibly and in detail with the history of the Soil Survey. But it remained available only in the Harvard Library. After transferring to Beltsville as Dr. Simonson's deputy late in 1957 he became ill and never got it whipped into shape for publication.

After a considerable effort I got a policy memorandum signed to provide a two-week training session in soil correlation by each principal correlator. A memo went to each soil conservationist in August with details about what kinds of men to select for the training. Fortunately, each of the principal correlators had had teaching experience. In addition, Dr. Simonson and others helped out as time went on. The first ones were scheduled for the following winter. These training sessions were a great boon to the potential soil scientists who wanted to learn.

I spent the week of September 1 at a meeting of our state conservationists in Asilomar -- a little resort place west and south of San Francisco. The meetings were not very good but better than previously. I had many helpful discussions with individual state conservationists. I then felt that my personal relations with nearly all of them were excellent and they continued to be. Yet this was certainly a poor place to have such a meeting. Of course our plane reservations got messed up on the way back. I never learned why Williams liked to have these meetings in out-of-the-way-resort hotels.

My back continued to get worse.

In September Louis Derr went back to his regular job as state

soil scientist in Oklahoma where he was doing a very good job and Roger Headly replaced him as assistant director of operations

Since the work load was building up we established an additional identical position and I thought to keep one as a training position to be filled by state soil scientists who would be benefited by the training. ⁵ Marshall of Texas had some good features but he was very weak in the basic soil science back of soil survey work. So I proposed he came in on a temporary assignment, which he had on August 13, 1957 to give some overlap with Derr.

In September the principal correlators were finally reallocated from GS-13 to GS-14. After that I was very slowly able to move some of the best of the senior correlators into GS-13 positions as assistant principal correlators.

Near the end of September my physician began a whole new series of tests.

Also in September I was having much correspondence with my friend ^W T. W. Schultz about his "homestead's in reverse." I explained in some detail how he was oversimplifying this whole problem of rural poverty in economic terms. After all there were home ties, family pressures, and other complex problems. On many farms the functions of laborer, landlord, capitalist, and manager were combined in one man. Most of those who failed as managers lacked skills to take city jobs. It was to take Dr. Schultz a little time to get off this binge. But he finally did.

For some reason Bill Johnson and John Barnes had a misunderstanding but Bill is the abler man and finally got it straightened out. The question was over excessively long connotative symbols. This was not fully resolved until about seven years later.

As the state conservationists began to analyze their costs they finally saw what we were talking about.

Billy Ligon began to worry about getting someone in his office to take his place. We both agreed that Dr. Bartelli, the state soil scientist in Illinois, would be an excellent choice. The steps were put in motion to bring this about. It took a long time.

On September 30 I gave a radio talk for the Department of Agriculture on soils.

Early in October my physician felt that I should stop the butazolidin. He gave me some ulcer medicine to prevent one coming. Gradually this helped the stomach pains but the back got worse so I had to grind up aspirin and take it in milk.

In October I arranged for Dr. T. B. Hutchings to represent the Soil Survey at an FAO-sponsored soil meeting in Pakistan.

On November 6 I went by train to Urbana and talked to the local chapter of the Soil Conservation Society on Changing World Agriculture. This trip gave me a good chance to talk with Ableiter, Bartelli, and others. I interviewed Dr. Robert Crossman who should like a job in our laboratory. Later this was arranged and a most fortunate appointment it was. Dr. M. B. Russell talked to me about my interest in becoming a "distinguished" professor in soils at Illinois and I told him that I couldn't see it for some time at least.

During the week of November 13 we had our national Soil Science Society meetings in Atlanta. I spent much of my time with the committee for the 1960 Congress and with the Tour Committee. Secretary Benson

spoke at this meeting at length about raising farm income by lowering farm prices! On the whole it wasn't a very good meeting except for the excellent side conferences in which I spent much of my time. Even Fowler and Mark Baldwin came. When the meeting was over I suddenly realized that stores were open in Atlanta on Friday evenings. On top of that it had rained. The traffic was packed and no cab could be found. So I got out into the middle of the street and asked people if they were going toward the station. A man in about the second car, with his wife, invited me to ride and even insisted on taking me to the door of the station, although from their conversation it was clear they were going somewhere else. I ran through and just made my train. Atlanta is a very friendly city.

Between letters and speeches and so on I was studying all I could about India because I was still planning to make the trip, difficult as it would be.

During the year I corresponded with Peter Mye of Ghana about a visit to the United States. Originally he wanted a tour. I suggested instead that he try for a place at North Carolina State at Raleigh where he could do research. They wrote to me about him and at my urging accepted him with \$125.00 per month as a stipend! So Peter would have this in addition to his Commonwealth fellowship. He was to come in January about the time I would be leaving for India.

On December 3 I flew to Tucson, Arizona for a regional meeting of the western state conservationists and specialists. I talked with them about the Soil Survey and our opportunities for making them better. The local boys had arranged a fine field trip. I

discovered this was managed by Al Swanson, the area conservationist, who was an old student of mine in North Dakota. The technical guide was the best one I've ever seen in SCS. They made a copy for me which I kept on my desk for several years to show what could be done to put our soil surveys to work. At this conference the word had got around that my back was very bad and that I was taking three aspirins every two or three hours. (To do this I went to my room, ground them and took them with milk.) But during the conference I was munching the odd gelusil. Later, one of the old SCS men whom I had never met came up to introduce himself. He said, "I want to meet you. I've heard for years how tough you are but any man who can munch aspirin tablets really is tough." This is the way stories get around!

After I returned I went to see my physician again and told him that I simply couldn't stand this regime and go to India next January 1. He told me that I had some symptoms of an ulcer but no ulcer and that I could go back on the butazolidin with some medicine to control the ulcers. What a relief!

After my return Monday, December 9 I tried to get things ready for my leaving and to read all I could about India. Of course I had to get my passport, visa, and all the rest to be ready to leave January 1, 1958.

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On New Years Day I left for India. During a brief stopover in Rome I had a nice chat and tea with the Ignatieffs.

This journey is covered in detail in India Journal, 1958. On the return I stopped over for about 18 hours with the Ignatieffs. I was again back home in the afternoon of Tuesday, February 11. Arrangements had also been made for Dr. Guy Smith to have a short trip to Chile this year.

The load at the office was heavy and all the pictures and notes on the trip to handle! I had a welcome note from Macmillan that my book The soils that support us, was going into its 9th printing.

I think my physician was a bit relieved to learn that I had had no trouble with back or stomach.

Very shortly after my return from India I was told that I was to head a mission on soil and water to the Soviet Union the coming summer. The Agricultural Research Service had several people going and I arranged with Cornell and the Rockefeller Foundation for Dr. Marlin Cline to join the group. So we were seven all together: Bulik, Allaway, L. B. Nelson, D. W. Thorne, W. W. Donnan, Cline and myself.

A year or so after this arrangement I learned of an extraordinary suggestion made by Don Williams during the conferences about this proposal while I was in India. A meeting was held among the administrators of ARS, Foreign Agricultural Service, the Secretary's Office and others. When the matter of the chairman of the mission arose Don Williams suggested Gladwin Young. Shaw and the others said that

the Soviets and other folks would never be able to understand why the United States would send such a mission without Kellogg as chairman. "He is an internationally known soil scientist and to make any one else chairman would raise all sorts of suspicions." Williams was reported to have said, "Well either one, it doesn't matter to me." This sort of attitude explained the troubles I was to have subsequently.

It certainly was a job to get on top of my correspondence and dictate the Journal of the India trip and sort out the photographs. Then too, many groups wanted talks about the situation in India and I had to prepare for our hearings.

I also found another problem. Red Smith, the state conservationist in Texas and Dr. Aandahl, the principal soil correlator for the Great Plains, wanted Marshall kept in Washington! A much better man was available for state soil scientist in Texas. I was shocked and amazed to find that this had been fixed up! I had a real rough struggle with myself. Several better men could have been selected. To keep Marshall, Hockensmith, Red Smith, A. Aandahl, and perhaps others from being mad, I let it go. But in the SCS, the only place to take care of poor men is the Washington office. We had a much poorer man than Marshall -- Henry Adams -- that Williams insisted I take even though he could not earn 10 percent of his salary! Such matters were badly handled. We had a very poor personnel officer and staff who had no concept of standards for scientists; ^{had SCS} for any goals for trying to get the most competent people available.

I spent the week of February 23 in Milwaukee making an inspection of both the Engineering and Watershed Unit and of the Cartographic Unit.

Ellis Hatt from Portland and Onet Frances from Washington assisted me. Ellis Hatt turned out to be a very able man. Lots of interesting questions arose, especially about Service work in the suburban areas.

Along about this time I made a determined effort to complete my collection of the Agricultural Yearbooks, which I was able to do from the second-hand stores and with the help of Foster Monmarat. Some volumes were in rather bad condition so I had to acquire skills in book repair, which stood me in good stead in later years.

This year we had an interrupted hearing before the House committee with little sessions on March 3, 5, and 7.

By keeping right at it I finished the India Journal before leaving for St. Louis on March 22 for the National Work Planning Conference of the Soil Survey. This ran through the week of March 24. (Insert 718a, 718b, 718c, and 718d.)

Shortly after my return things began to happen. D. A. Williams renigned on his promise of about a year ago regarding the soil correlators, which I had already anticipated he might. On April 2 he sent me a memorandum that would reorganize the work drastically, downgrade the senior correlators, and place them under the state conservationists! I wrote out a full memorandum of the principles involved in this work, which he ignored. Obviously, he can't understand or doesn't want to, the highly scientific character of this work. It was clear that the plan had been cooked up during my absence in India. At that time it began to be clear that this scheme was probably discussed with Hockensmith who has a weak personality when

March 1938

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At this one I emphasized my hope for more effective contributions from the State experiment stations. Yet we had markedly stepped up our progress in mapping. Mapping with standard legends were quite a bit faster than mapping with the old SCS legends, partly because more of the standard surveys were progressive and partly because the legends were much clearer and easier to follow.

By this time we had made good progress toward revising the statements of assumptions, criteria, and definitions on the categories for the capability groupings. Now we need much more emphasis on soil handbooks. These were really expanded descriptive legends that included the interpretations. Despite all of my emphasis on interpretations I explained that I was finding county after county without any handbooks because the interpretations had not been developed. Obviously, farmers in those areas were not getting up-to-date assistance in farm planning in accordance with our statements. We also reviewed the growing evidence of the rapid expansion in the use of published soil surveys. This was especially true in certain urban areas and where the State college and the Service were making general introductions of the published soil surveys to the people in the county with a regular educational program.

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Drs. Smith and Ruhe talked about some of the new lines of research.

We had some useful discussion on the selection and emphasis of benchmark soils. The purpose of this was to concentrate our research and study on some of the principal soils with the thought that by studying these intensively and having a full set of data we would make more progress than by scattering the same effort widely. This we had discussed previously as one of the principal responsibility of the regional committees. Some work had been done but more was needed.

We also went over the facilities and opportunities for training, which still needed considerable emphasis.

Several trials on the use of published maps in farm planning were reported. The only real difficulty involved was the one of training soil conservationists in a slightly different way of doing our work. This turned out to be an unnecessarily big job. We seemed to have no problem with the engineers.

Mr. Orvedal reported on the progress made with engineers in the use of soil maps and for the preparation of suitable engineering chapters for the published soil surveys. This was soon to become one of the most important aspects of our work.

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Mr. Orvedal presented the best small-scale map that we had. This was a map of North Dakota on a scale of 1:1,000,000 with a legend that made it possible to read clearly two levels of generalizations.

Near the end we discussed the difficulties and costs of having both the regional conferences and the national conferences every year. We were particularly anxious to have a solid progress statement on the new system of classification for presentation at the International Congress in Madison, Wisconsin scheduled for 1960. The question was discussed and left open for the moment. (Later, we decided to skip the meetings for 1959 and 1960 and have the national conference on odd years and the regional conferences on even years.)

We did plan to have some committee meetings and meetings of the principal soil correlators anyway.

A committee on Horizon Nomenclature had presented a scheme that was altogether too complex for field work in the absence of firm laboratory data, so I asked Drs. Cline, Smith, and Simonson to take a look at it from the point of view of the relatively experienced soil scientists, the experienced soil scientists working rapidly in the field, and the skilled soil morphologist with plenty of time and supplementary laboratory data. It seemed that this committee had gone as far as it could as a committee of the conference.

We urged the men to get more yield data by soil types on ranges as a basis for planning range surveys and interpreting them. We had a similar problem in forestry but somehow we were making better progress there.

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the chips are down and who may possible have favored it in the good old SCS spirit of cronieness and respect for long and faithful service, which doesn't mean many years of experience but means the smae experience year after year.

I got slammed down real hard on this in a memorandum from Williams dated April 11 for unbelievable reasons. I suppose I should have offered to resign, which may have been the intent of the memorandum in the first place. But this would have been too easy for me and far too hard on the 25 or so top men in the Soil Survey who were depending on me. Most of these would have left and the Soil Survey would have been in ruins. So I decided to go ahead with this unknown potentially inefficient procedure. I had fought the green goddess before and I supposed I could do it again.

I wrote about the situation frankly to each of the principal soil correlators as well as a personal letter of caution to Bartelli that we would go ahead with our plans later. I asked the principal correlators to use their own judgment in explaining the situation very confidentially to the good senior correlators whom they could trust. The principal correlators were both shocked and splendid. They wrote me fine letters of encouragement that helped a great deal. And so matters rested for awhile.

As time went on I explained the whole situation very confidentially to people like Dr. Roseman and certain members of my senior staff in Washington.

Sunday evening April 27, I started another visit to South Carolina

at Dr. Buie's request. He had asked me to look at some of the work in the western part of the state. We had a fine joint field study with the experiment station people of the National Forest Service. They had discovered many important relationships between the diseases and growth of various species and the highly contrasting soil conditions. We also had a conference at Clemson with the college people following which I had a frank talk with Buie. He was deferring too much to a no-good soil "scientist" by the name of Miller whom someone had promoted to assistant state soil scientist despite his complete lack of qualifications.

Earlier, when Montgomery left, Buie had not wanted to take a man from the outside as state soil scientist so I settled on Ellerbee a first-class area conservationist even though Billy Ligon objected. He had a rough time for awhile but he turned out all right.

The evening of May 1 in the little hotel on the campus at Clemson I had a long talk with Dr. Vince Cardon. I told him about Director General Sen's insistence that I come with FAO. Then he poured out to me his terrible problems as Director General of FAO -- about the trouble he had with Gove Hambidge, who had had many special privileges and wanted more and about Gove's mishandling of funds here in the regional office. He went on about many bureaucratic squabbles including those between Ralph Phillips and Harold Vogel for the position of North American representative following Hambidge's "retirement." (During his last years Hambidge had written a book on FAO. He sent me a copy. It was pretty awful. He asked me about it and I told him that his concept of FAO were so different from mine

that it was difficult for me to evaluate it. He hardly spoke to me after that. He retired a bitter, unreasonable man.) Cardon also explained that he had essentially no support from the American Government, only criticisms and heckling. It was a sad story from a wonderful man who lacked the skills of administration.

I returned to my office on May 5 and the advisory notice reorganizing the soil correlation work, which had been hinted, came out May 8. Yet at that moment Don Williams seemed a bit more reasonable. But this didn't last long. The following day, May 9, however, he agreed to my suggestion of the addition of eight assistant principal soil correlators, again verbally.

On top of all of these problems I had to plan the details of the excursion in the Soviet Union, arrange tickets, visa, and so on.

On May 21 Don Williams completely reversed his statement of May 9. Now he said that we should have the same number of senior soil correlators with only one assistant principal correlator for the Great Plains. I pointed out that I would rather have the funds for my office reduced if necessary in order to have another in the South and especially in the West. He was firm against it. He claimed that it would give the impression that we didn't expect the new plan to work, which it didn't. Again I kept the principal correlators informed. They couldn't help but be discouraged but they didn't want to hurt my morale.

A flow chart was prepared for the signature of Mr. Peterson.

Also along about this time we were having a great deal of trouble about our Library. Like most librarians Foster Mohrhardt has a

strong personality when things are going well and a weak one when they go badly. Ralph Roberts, the Assistant Secretary for Administration hated books and considered himself an expert on everything. The Civil Service Commission forced the Department to bring the grades of the people in the Library in line with the jobs. To get the money to do this he forced the Library to cancel the contracts with the land-grant colleges to supply books and journals to the SCS men in the field who wanted to read. The costs of these contracts were low and the service excellent. I was the representative for the SCS and each of us from the agencies agreed to transfer funds to the Library rather than take on this job of buying and circularizing professional journals. But Roberts wouldn't listen.

On May 26 I had a long conference with Assistant Secretary E. L. Peterson about whether I should take the position as Assistant Director General of FAO for Technical Services. Dr. Paarlberg, Dr. Shaw, Dr. McArdle, and Assistant Secretary Roberts had been urging me to do it. Mr. Peterson stressed at this time how badly my counsel and services were needed in the Department.

He asked me about progress. I briefly explained the plan which put the senior soil correlators under the state soil scientists and thus downgraded the scientific work of the Soil Survey. He was obviously amazed and shocked! Then so was I! I explained that I had been told that this plan was his idea. "Absolutely not," he said. He went on to say that he had said that the state soil scientist should be responsible for the use and interpretation of the soil survey within the state, but that he had never dreamed that he would be responsible for the scientific work. He asked if the plan had been announced. I said it had; that the difficulties for

recruiting scientists would be well nigh insurmountable; but that it was too late to go back. He insisted that he was misquoted; that he had signed nothing; and that he would ask for a full clarification. He further explained that he would regard this as a confidential and privileged conversation. I am sure that he meant this at the time but I am equally sure Williams found out that we had talked. I had had no intention of going over Williams on this matter. I had called on Peterson about the FAO matter at his request. At this time I thought he might ease things a bit but I doubted that he would have the courage to order Williams to do the right thing to maintain scientific standards, at least not now with an election coming up in the autumn.

Obviously both Williams and Young purposely misled me. I knew then that the whole plan started in the SCS staff. Obviously the motive was jealousy of the high scientific standing of the 25 or 30 top soil scientists as compared to nearly all the other people in the SCS. I had been naive enough to hope that instead of being jealous they would be proud of the Soil Survey. Actually most were. But not Williams, Young, Norton, Graham, and a few others among the "old timers."

On May 28 in a conference with his immediate staff and the policy committee of state conservationists, D. A. Williams again referred to Assistant Secretary Peterson's suggestion for the change in the organization of the senior soil correlators." So the plot thickened.

On May 29 we had the second day of the administrative conferences with Williams and his immediate staff, field representatives, and the policy committee. This was the same group that he told me on another occasion recommended the downgrading the senior soil correlators.

Most of the talk was about marshalling political support for funds and strategy against the old AAA, then the CSS, and later the ASCS. These boys had organized an outfit called FARM -- Farmers Association for Resource Management -- which was trying to get soil conservation districts voted out by attacking the weak ones. Everyone, except myself, assumed that our technical work was excellent. Actually we had many weak districts and many weak state programs.

This was followed by talk of how to increase grades, especially for Van Dersal (who was there!), the field representatives, and state conservationists. The whole talk was very shallow.

Near the end, it was explained that any attempts among state offices to share technical people never worked! Yet this has just been decided for soil correlation -- which is more technical than anything else in the SCS! It was also explained that an area conservationist could not even administer the engineering phases of watershed work unless he was an engineer. This was silly. Of course he can't make technical decisions in engineering or soil science but a good one knows this and gets help with the technical phases while he rides herd on the schedules. D. A. Williams was so upset about his current bureaucratic struggle for power with ACP and

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CSS that he has lost his balance and hit out in all directions, arbitrarily and emotionally.

It took a lot of preparation for the Soviet mission, even including medicines. (It worked out that I didn't use many of these but the other boys did.)

About June 12 I had a conference with Director General Sen of FAO. I explained my philosophy of FAO, which I wrote into its constitution in Quebec in 1945. This was the trouble with Hambidge's book which dealt only with the nameless secretariat. I was much opposed to the statement, "an FAO expert" does so and so. For such reports to mean anything we need ^{the} ~~to~~ person's name and his or her field of specialization. He agreed. Then too, the lonely research worker staying within his own country is a part of the FAO concept. "each country is to help themselves and one another." But then I also took up with him the head of forestry in FAO who was a poor forester and untruthful. He said abruptly, "We've got to keep him." This helped my decision.

Sunday, June 5 I went to Memphis for a couple of days to take part in a conference on a rural development program. Shuman of the Farm Bureau recalled the County-Planning Program with derision and said that a federal-state rural development program would come to the same end. This fellow was awful. Although national president of the Farm Bureau he is either an economic illiterate or a hired hand of big business, or both.

I expected to see Louis XIV come out from behind the curtains smiling. In the session Director General Sen gave by far the best talk.

From most of the other speakers we had the reactionary conventional wisdom which assumes that farmers are simply the hired hands to furnish cheap food and low-cost labor to business.

But in the small group sessions the talk was exactly the same as it was during the early days of county planning. Bushrod Allin and I had the same reaction: "This is where we came in."

On July 19 Dr. Nyle Brady called me about this demoting of the soil correlators. He asked if it were my idea? I told him, "No." He asked if it were a threat to the scientific standards. I told him, "Yes." I could only explain that a joint committee on soil survey with the Department and the colleges could be helpful. The Department man would need to be from the Secretary's office. He said he would ask Dean Myers to discuss it with Peterson.

Later I talked very briefly with Peterson and told him about this. (Williams was away.) I pointed out that the dean might call on him. I gave him a copy of the advisory notice of May 8 signed by Williams. He appeared to be angered and said he hoped Dean Myers would call. That evening Dr. Bradfield called and I told him that Peterson would like a call from the dean.

Early in July Dr. Knoblauch called and told me that the experiment station directors were about to organize a Soil Survey Committee urging a revision of the advisory notice. On July 2nd I told Mr. Williams that I had heard this rumor and he told me that Dean Myers had given Assistant Secretary Peterson an intemperate memo by Marlin Cline, "cussing out the Service." Why of why would

Dean Myers do such a foolish thing. (I talked about it later with Brady. The dean had asked for an "information memo" but it never occurred to Brady or Cline that he would give it to Peterson. And why did Peterson show it to Williams?) Williams told me that, "Pete is not disturbed about the letter and is writing an explanation to the dean." Oh God! Oh Montreal!

Later I saw a copy of this memo from Brady and Cline to Dean Myers. Each individual on the Experiment Station Committee on Policy had a copy. There was nothing intemperate in it. In fact it was a fair objective statement of the situation.

July 7 Knoblauch showed me a letter from the Assistant Director at Cornell quoting Mr. Peterson that he would see to it that the reorganization of the correlation staff would not go into effect. What a mess. It had on July 1. Peterson failed me. The pressure must have been terrific and was probably political.

Then on July 8 Brady called and I made three recommendations at his request: (1) That any USDA-Land-Grant Committee must include someone from the Secretary's office; (2) the full plan should be explained to Dr. W. H. Pierre of Iowa; and (3) Dean Myers should gently prod Peterson just so he doesn't forget the matter. Then he told me that Dean Myers did not say that Peterson would reverse the order, rather that he would explore the matter and perhaps reverse the order.

On July 14 Williams reviewed with me and Gladwin Young the steps he said he had taken on this reorganization of the correlators.

1. He said he had problems reported to him. (None were specified at any time.)

2. He said that he had talked the matter over fully with Peterson and was told to go ahead with the change but he wanted to wait. (This conversation was alleged to have taken place while I was in India.) If it did Peterson didn't know what Williams was talking about.

3. Williams said he discussed it with the policy committee of state conservationists and they unanimously approved it. (These were Buie, Edminister, Koyle, Davies, and Kenard about the weakest in the whole Service.)

4. He said he discussed it with Kellogg who raised questions and finally agreed to go along.

5. He issued the directive as advisory notified W-116 dated May 8, 1953.

6. He asked the state conservationists to discuss it and the state experiment station directors. Nearly all said no serious questions arose.

7. Dean Myers of Cornell raised a question about it to Mr. Peterson together with a memo from Brady and Cline.

8. Williams read a letter that he said Mr. Peterson sent to Dean Myers explaining the purpose of the move.

9. He said that he had a talk with Dr. Knoblauch and toned him down.

Williams then suggested that I and my senior staff were not supporting the change. I denied this and explained that the men were very worried about the work. Unhappily, I had apparently misunderstood his reaction to my proposal for 3 assistant principal soil correlators. I had understood him to say that he had agreed

to this and found only later that he had not.

Obviously things didn't add up. But I wrote a memorandum to the directors and principal correlators with a copy to Williams explaining how we would try to make the new organization work and admonishing them to be careful in their discussions.

It became clear that E. A. Norton had a lot to do with this. For many weeks I had been hearing rumors of his statements that soon both interstate soil correlations and soil survey publications would come to an end. Large groups were told this in New Jersey in December 1957 and in Maryland and Virginia during March and early April, 1958.

I had never learned specifically who was back of this and for what reasons. Obviously if Williams had really heard of any difficulties he would have discussed them with me. Actually there were none. He may have heard some planted gossip or stories but certainly not anything real from people who understood the work. He may have got some support, or at least lack of opposition, from Roy Hockensmith who had little understanding of the scientific methods involved in soil correlation.

In his recent conversation Williams said some of the senior correlators may be made GS-13 state soil scientists when the new standards we had been developing came out. This is the ridiculous argument that Hockensmith had been using that showed his lack of understanding of what was involved. No state soil scientist however competent would have any significant amount of time for interstate correlation. Furthermore, it was absurd to have state soil scientists in GS-13 and soil correlators able to do the work in GS-12 when the latter job requires much more scientific skill and knowledge than the former.

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Meanwhile the statistical study of the use of published soil surveys was going forward by AMS.

On July 16 our mission members assembled in New York City for the excursion in the Soviet Union, except for Dr. Thorne who would join us in Brussels. We were away in the afternoon. As the scheduled worked out we had nearly a day in Brussels which gave me a chance to see the World's Fair there and to have a good discussion with J. Jurion. I asked his judgment about the position in FAO. He argued strongly against it. He said, "If you want to make any trips I know that any one of a dozen governments that would be glad to arrange it. (I do not know where he gets his information. He had told me earlier that if the United States had put my name forward, I could have been elected Director-General of FAO instead of Sen.) "Further," he said, "you can have more influence from your well-known position in the USDA than you could in FAO."

Later when we came to Moscow I found another urgent letter from FAO asking me to accept the position.

The full details of the visit to the Soviet Union are set forth in the two volumes of the Soviet Journal so there is no use repeating them here. In addition the SDA published our official mission report.

I returned to the office August 22 after a long and strenuous journey but certainly a very interesting one. Two days later we had a press conference about our trip. Most of the boys were looking for something critical although they didn't get much. But the reporter

or editor can always manufacture it. I explained, for example, that the mission arrived in Moscow just at the time of the demonstration at the US Embassy about the American troops going into Lebanon. Thus the Embassy people did not meet us at the airport, yet there was a large and friendly Soviet delegation to meet us and take us to our hotel. These were people from the Academy of Sciences of the USSR and from the Ministry of Agriculture. Some of my friends heard the radio newscast. They used only the first part of what I said, thus creating the impression that we were standing helpless without anyone to meet us.

I was almost too busy to worry much about soil correlation with the Soviet Journal and report to do. I did work in a short conference with the principal soil correlators on plans and troubles.

Peter Nye had done well at North Carolina and was leaving in September to go back to Ghana. Actually he didn't stay there very long but joined the International Institute of Atomic Energy in Vienna. Then after a little while there he went to Oxford University when Scofield died.

I worked rather furiously to get my paper ready for Michigan State University. About a couple of years earlier I had urged them to call a national conference on soils and forestry, which they did. I spent September 8 to 11 at East Lansing and gave the lead-off paper, A look at future forest-soil problems. I also took along my Soviet slides which had turned out very well indeed.

The second edition of The efficient use of fertilizer was about to go to press. Orvedal and I fixed up a revised soil map.

Then I had another letter from FAO urging me to come, which I turned down as gently as I knew how.

I also had a showing of the slides in the Secretary's conference room. I think these made it clear the the Soviets weren't ~~as~~^{so} bad as most had supposed from the press.

Early in October while I was working feverishly on the Soviet Journal and report in came Dr. Douglas Ensminger inviting me to go to India for three months with a team being financed by the Ford Foundation. I explained to him that I had had two long, hard trips to Asia during the year and that I was tired. He was persistent. He asked, "Would the Department let you go?" I replied, "I don't know and I'm not going to ask." He offered me a generous fee. I explained how I needed the treatments for my back. Finally he said, "I will go and ask Mr. Peterson, ^{and} the Foundation will pay the travel for you and your wife and let you take stops enroute and return." I thought I was safe in agreeing if all these conditions were met. But he was a persuasive man and they were met. Now I really had to get at the Soviet Journal and report.

October 3 we had good budget hearings on the Soil Survey with the Bureau of the Budget. For quite awhile I spent much of my time, especially week ends on the Soviet Journal. My wife helped and got some practice for the next year.

^H The state conservationists had their meeting here the week of October 13. It was the usual talk about how good the Service was and how we should improve our management. Yet not many of the state conservationists

were enthusiastic about the new correlation program although I could see that each one wanted his own man thus leaving the principal correlator to do all of the interstate work. He simply couldn't do it without more help. Some agreed to have their men work a little more in other states.

October 20 to 22 I attended the meetings of the Soil Conservation Society in Ashville, North Carolina and made an address on the Role of the scientist on shaping land-use policy. The highlight of the meeting was a little side trip to visit a museum on Thomas Wolfe. It was the old boarding house where he lived as a boy and which is exactly described in Look homeward angel.

I came back and worked as hard as I could on the Soviet Journal and then went to Ames, Iowa for the week of October 27 for a large conference on American agricultural policy. I gave a paper on Prospective land use and adjustment: Opportunities and limitations. All the papers at this conference were published as a book by the Iowa State University Press. I had a lot of fun here. I took along my Soviet slides for an additional evening lecture that was exceedingly well attended. Ted Schultz, John Black, and I had lots of fun and argument. Ted jumped all over Ken Galbraith's Affluent society. I think the trouble was that his economic friends felt he should have published these ideas in formal papers for them to criticize before writing a popular book for the public. Since the conference was published as, Problems and policies of American agriculture there is no need to write anything here about the papers.

On November 4 we had a general election and the Republicans lost the Congress. But I thought that probably President Eisenhower wouldn't worry too much. He hadn't yet.

About November 6 Peterson and Ralph Roberts called me over, tried to butter me up real good, and suggested that I take the job in FAO for two or three years. They said that my position would remain open in the Department and that FAO would make the contributions to my retirement fund just as if I were here. I told them that I would think it over again. Then I asked them to reconsider this decision on the Library and the circulation of journals. "That's all settled and we won't reopen it," said Roberts. If the Secretary's office wouldn't take my word on something like this I knew I would get no backing in Rome. I stopped by Dr. McArdle's office on the way back and he gave me some good advice. He wanted me to go. But after I explained the terrible bureaucratic situation in SCS he could see why probably I shouldn't. He said, "If you decide not to go don't argue with Peterson and Roberts, just write them a little note that for important personal reasons you don't want to go. This I did and that finished the matter. My only sorrow is for Ignatieff who was so anxious for me to come. Then the Director General very foolishly, offered the job to Frank Parker who took it. He stayed there for 2 or 3 years before they caught on.

Tuesday evening November 11 I left by train for Nevada to address the State Association of Soil Conservation District Supervisors on the Uses of the Soil Survey at Winnemucca, named after an Indian princess. The through trains pass this place in the

middle of the night. Like other Nevada towns there are many slot machines in continual use. I think I had quite a compliment after my speech and question period, which ended just before a recess. As I came down the platform I noticed a group of ranchers standing around arguing. They all dress alike -- ^{the} cowhands and the millionaires. As I came to the back, I heard one say, "Well, I'll just ask him." He came over to me and said, "I want to ask you a question. Have you ever managed a farm?" I told him that I had years ago. He turned to the others and said, "I told you so." So I went up to the group and asked them what this was all about. One said to me, "Several of us felt that you must have managed a farm because you're the first so and so out of Washington that we can understand."

We had a field trip with about 4 inches of snow on the ground. I was sure that if I hadn't have been there they would have skipped it.

I was able to do some writing on the train and fairly well handled my correspondence when I got back. I worked in a nice conference with Charles Hardin between trains in Chicago.

Sunday, November 30 I took the train to Indianapolis. The train was very late and I went in a Service car to Lafayette for conferences with our staff. I certainly had a work out the first day. I had seminars on soils and climate, one on teaching with the younger professors in soils, another with the soils extension people, and finally a lecture on the history of changing ideas about soils and soil classification. The next two days we had meetings of the field soil scientists in the state. These were not very good.

Neither Bass, the state soil scientist, nor Grunwald, the assistant state soil scientist, was competent. Some of the party chiefs were much better. I talked to them about a balanced program, answered many questions and corrected some serious errors. The first evening I gave a slide lecture on the Soviet Union.

The last day, December 5, Lester Binnie, the assistant state conservationist, took me to the train. He didn't make a good impression beyond being a loyal and faithful employee.

I worked hard during the rest of December getting the Soviet Journal and report finished and preparing for a new trip.

I made a design of the baker I use for back treatments and sent this with photographs from different angles to India so that one would likely be ready when we got there. It was quite a job to sort out and plan the baggage for the two of us for four months.

On the whole this has been a good year for my education and a bad year for the Soil Survey. Assistant Secretary Peterson finally approved the organization chart even though he told me he never would. He should have realized that the Republicans would lose the elections anyway and not have tarnished himself.

I felt then that I made the right decision about FAO. The United States Government never had supported FAO, only complained. They were looking for someone to rebuild US prestige in FAO and get them off the hook.

Near the end of the year Williams calmed down a great deal and became much more cooperative. I suspect that Assistant Secretary Peterson gave him a good blistering but I don't know.

